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ERRATUM

In the article by Elof Axel Carlson, "Comparative Genetics of Complex Loci," March, 1959, on p. 34, line 35 in the lefthand column has unfortunately been completely substituted for the original statement.

The final sentence of the second full paragraph should read as follows: "The components of the gene which are separable by recombination are called subgenes, subloci, or *mutant sites*, depending, respectively, on whether one is referred to the gene concept, the linkage map, or the hypothetical molecule which composes this hereditary structure."

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THE QUARTERLY REVIEW of BIOLOGY

THE ORIGINATION OF LIFE

BY A. E. NEEDHAM

Department of Zoology, University Museum, Oxford

ABSTRACT

A formal solution of the main problems concerning the origination of life is attempted, adopting as fundamental axiom the plausible assumption that it was a spontaneous, natural sequence of "most probable" events. Survival by natural selection is a particular example of a most probable event, and therefore operated at all stages of the origination. True evolutionary novelties have been most probable responses to new environmental conditions, and may have become rarer as the rate of change of the causal conditions decreased.

The most probable behavior of living organisms, as "open" systems, differs from that of classical, closed systems. The steady state, characteristic of such open systems, involves the apparent paradox of a stable, self-perpetuating "fabric," maintained by cyclic metabolic activity. The stability is dynamic. Theories on the origination of life frequently consider only the fabric and neglect the metabolic aspect of the system. This is facilitated by several factors and can lead to errors, particularly in scales of rate and magnitude. A slow, protracted acquisition of new materials, even represented by a single molecule of its species in some instances, is often postulated. It is more probable that all significant materials and reactions were acquired early, panglobally and in quantity, and that subsequent evolution was restricted (a) to most probable innovations, as indicated above, and (b) to less fundamental taxonomic changes, depending in part on changes in the biological environment itself. In general there has been biochemical simplification during evolution, rather than the converse.

An "ametabolic" view leads to the questionable conclusions that there were originally no autotrophs, no photoactivated endergonic syntheses, no need for solar energy, and no decay, that the initial heterotrophs could feed indefinitely on a limited store of pristine organic compounds, and that generally reducing conditions prevailed on the early earth. This view fails to recognize that the general level of oxidation is less important than the maintenance of a potential difference in free energy between organism and environment. It also tends to overlook the relative rapidity of the circulation of organic material through living systems, and the evolutionary implications of this. The cycle is not related to the diurnal or the seasonal cycles of solar energy influx. It is a sine qua non for continued life. Any serious "bottlenecks" or "stockpiling" of materials could be disastrous. There has been automatic selection in favor of the cyclically interdependent autotrophic and heterotrophic activities, since indirectly each is self-promoting. This has led to increasingly rapid circulation, but with limiting factors. The rapid circulation of material, manifested as an intense competition for it, indicates that material, and not energy, has probably always been the limiting factor in vital activity and evolution on earth. In this open system, energy supply is probably not a serious problem and endergonic reactions are not highly improbable. Molecular aggregation, i.e. synthesis, and

similar phenomena at the higher, organismal level—ingestion, syngamy, etc.—may be most probable processes in such systems, since they could thus absorb surplus energy. Selective aggregation is often observed in both inorganic and organic systems. The formation of "coacervates," pristine discrete organisms, is probably a spontaneous process of this type. Losses by wear and tear are an inherent, most probable property of any system and an effective, statistical replacement was a first essential for living systems. With the emergence of discrete organisms more specific replacements became necessary. The phenomenon of death also appeared, as a discontinuous macro-loss, and reproduction and growth emerged as the appropriate replacement. The various "work-functions" of living organisms are basically similar and have much in common with the aggregation-ingestion types of most probable processes.

INTRODUCTION

THE problem of the origination of life still presents great difficulty, and the most satisfactory form of review of the extensive, and very stimulating, recent literature would seem to be a purely formal approach, aimed only at the main questions. So long as there is uncertainty about the answers to these, attention to detail is scarcely justified, even if space permitted it; moreover, it would tend to obscure the main issues.

The extent of the uncertainty also necessitates an axiomatic assumption of the answer to the most fundamental question, namely, whether or no the origination was a spontaneous, "natural" process, requiring no agencies or properties other than those which govern the behavior of non-living systems—whether or no "... everything in the behavior of living matter is explicable in terms of physics and chemistry ...", as Bertrand Russell puts it (see Branfield, 1950, p. 116). The whole problem of the origination of life centers on this question. To obtain a certain answer to it inductively will demand a very complete knowledge of the early history of the earth (Urey, 1953) and of the chemical properties of living material (Oparin, 1957; Oparin et al., 1957). At present the alternative approach, essentially a deductive examination of the consequences of the fundamental axiom, seems justified. Its value may be tested by the degree of coherence of the account it gives, and by its plausibility in the light of available knowledge.

THE AXIOM AND ITS COROLLARIES

The axiom here adopted is that the origination of life *was* a natural process. The alternative sets no limit or guide to further speculation, and also puts the subject beyond the pale of orthodox scientific experiment (Pringle, 1953). It should be rejected, if for these reasons alone. The preferred axiom, on the other hand, both permits experimental testing and lends itself to the relatively simple formal treatment advocated above, for examining the further main questions. These questions, some of which manifestly are direct corollaries to the fundamental one, seem to be:

(1) Was the origination a slow, continuous, panglobal process, or was it a sudden, once-for-all, local event?

(2) Were the first living "systems" autotrophic or heterotrophic?

(3) Did they obtain a store of energy, as ultimately organisms do today, by photo-energized endergonic reactions?

(4) Did these reactions involve the formation of a relatively oxidized environmental medium and of relatively reduced materials in the living systems themselves?

(5) How did living systems evolve their property of self-perpetuation?

(6) How did discrete "organisms" arise?

(7) What is the origin and basis of the intense competition between living organisms?

As the first direct corollary to the main axiom that the origination was spontaneous and natural, it follows that no unnatural, "improbable" events were involved; individually and collectively biological events have always been "most probable" events in their contexts of time, place, order of magnitude, degree of organization, etc. Lotka (1925, p. 32) considers that the tendency towards a most probable state is practically universal and Eyring et al. (1958) emphasize that those reaction-paths most "favorable" energetically account for virtually all of the activity of any particular substance.

Bernal (1951, p. 37) considers that all steps in the origination of life were not merely spontaneously possible but in fact inevitable, and

most modern biologists believe (Gaffron, 1957) that life is bound to appear anywhere in the cosmos as soon as conditions become favorable (cf. Shapley, 1957; Oparin, 1957). To quote Joseph Needham (1936), "when cosmic conditions permit . . . matter produces in actuality what it has always had within it in potentiality." Pringle (1953) sees organic evolution as part of the spontaneous evolution of the universe as a whole.

No doubt an event which is most probable under a particular set of conditions may be highly improbable under some other set, and the conditions under which biological evolutionary events are spontaneous may be different (p. 197) from those which determine the behavior of inorganic systems. For instance, there is the now familiar contrast between the conditions of the classical "closed" systems and the "open," steady-state conditions of living systems (Prigogine, 1955; Oparin, 1957). In the latter, a local increase in free energy is by no means improbable, as it is in a closed system.

The conditions under which biological evolutionary events are most probable may be relatively uncommon, and these events themselves correspondingly rare among the totality of natural events. Some biologists, from Redi and Spallanzani (Pirie, 1957a) to the present day (Haldane, 1954a, 1957; Mitchell, 1957), have considered that life may have had an origin, not an origination—a single essential event, rare to the extent of virtual uniqueness. Such an event is sometimes called "highly improbable," in the statistical sense of being very rare, and while this event itself may be that which is most probable under its particular conditions, no doubt it is legitimate to call the latter "improbable" conditions.

An event which is rare owing to rather improbable causal conditions might still be relatively frequent on the geological time-scale and within the geographical space-scale, and the origination of life would seem to demand a succession of events of at least this degree of frequency. No doubt the perfection of even the simplest grade of organization which would be regarded as well-established "life" demands such a succession of historically, i.e. genetically, linked events. This seems the only reasonable origination of the most essential property of living systems, that of self-perpetuation. A unique, large, saltatory event—an "act of creation"—seems as incompatible with subsequent self-propagation as it is devoid of genetic roots.

At any stage of evolution the genetic continuity must be seen to extend back indefinitely. Madison (1953) and many others since T. H. Huxley, have envisaged a smoothly continuous evolution of living organisms from the simplest initial systems. These were not "inorganic," which would imply a discontinuity at some later stage, for the materials no doubt were always mainly carbon compounds, and indeed in essence they were already "alive." Ycas (1955) talks of a "metabolizing ocean" of these initial materials. J. and E. M. Lederberg (1956, p. 121) are among recent authors who favor an uninterrupted evolution from this state by numerous small changes.

If the origination was a natural and continuous process it most probably occurred entirely in situ, on earth; and, while keeping an open mind (Lederberg and Cowie, 1958), workers now generally accept this view (Oparin, 1957). Even if living "spores" could survive the rigors of transport from another world, on arrival they woud need conditions suitable for their particular form of life. It is probably true that conditions on earth in fact are not grossly different from those on many other worlds, but this applies, inter alia, to the materials available, and for that very reason it is at least equally probable that our life originated here, notwithstanding other possible originations, elsewhere (Wald, 1954).

It also follows directly from the basic axiom that natural selection has determined the direction of origination and evolution from the outset (Pringle, 1953) and not merely in its more recent stages. As Osborn (1918, p. 20) contended, "selection antedated the origin of life just as adaptation did." The validity of this extended operation of the phenomenon of natural selection can scarcely be challenged since, strictly speaking, it makes no more than the tautological, if also fundamentally important, claim that at all times the "viable" has survived (or persisted) while the inviable has died (or changed). It describes the natural course of events under a particular set of conditions, and is therefore a special case of the fundamental axiom of maximal probability. Under one set of circumstances the maximal probability is that one system or organism persists while another suffers that change which, once discrete

organisms had arisen, became a relatively sharp and complete disorganization, called death (p. 205).

According to the theory of "maximal probability" all events in living organisms, including changes of evolutionary significance, are most probable events in their contexts and therefore sometimes, if not usually, it should be possible to recognize the specific causal agents of significant changes in the genetic material. Unfortunately, this seems far from true of the classical mutations which are, or include, the only type of change in which most biologists perceive potential evolutionary significance. Mutations appear to be evoked very non-specifically by a wide variety of experimental agents, and each agent is able to induce a large number of the known mutations in any particular species. This seems to rule out any specific causal relationship between any one mutation and any one agent.

Many of these known mutations also occur "spontaneously" and with a relatively high and constant frequency which may imply causal agents with similar properties. By contrast, true novelty is expected to occur only once, or rather rarely, and there is no good evidence that evolution is typically reiterative. Further, the spontaneous frequency does not vary greatly among the various mutations, and this is statistically improbable on the supposition that each is due to a different, specific, causal agent.

Another familiar puzzle, and a difficulty for the present thesis, is that the nature of the mutation often has little "biological meaning" and even less biological value (Schmalhausen, 1949, p. v). In a modern living organism a most probable response might be expected to show something of these properties, since by this stage of evolution the potentialities for the more irrelevant kinds of change should have been eliminated by natural selection.

However, these are not necessarily to be considered serious difficulties so long as the significance of mutations, in the first place, and in the second the identification of true evolutionary novelty in the genotype, remains so uncertain. The extreme possibility is that most mutations are nothing more than trivial position-effects (Glass, 1957, p. 774) without much biological significance, or they may be stereotyped change-responses, acquired in the course of the evolution of the genetic mechanism itself (Darlington, 1958; White, 1954), with the useful function of maintaining a fund of variability in the genotype against the eventuality of a changeable environment. Any disturbing agent, whether natural or experimental, appears to trigger off the same repertoire of mutation-responses, some, at least, of which may be expected to give progeny fitted to any particular eventuality. Acquired responses of this kind, by analogy with the behavior-responses mediated by the nervous system of animals, may have come to be very indirect consequences of the evoking change in the environment, and are not expected to show the simple, most probable quality postulated for true evolutionary novelty. Behavior-responses, indeed, often have the useful function of diametrically opposing and neutralizing the action of the evoking stimulus.

Some of the recent work (e.g., Cohen, 1957; Benzer and Freese, 1958) tends to show that in fact mutagenic agents may have specific actions, e.g., on the rate of mutation and on the locus which mutates, and that the mutagens concerned are physiologically and genetically meaningful in nature, e.g., food materials, and nucleic acid components or their analogues. Some mutations at least, therefore, may be more direct and probable responses than the orthodox picture would indicate.

A further property of most mutations which at first seems to present difficulty is the magnitude of their phenotypic effects. They tend to cause saltatory or "macroquantal" changes, and this seems incompatible with the criterion for significant evolutionary change given above (p. 191), namely, that it should be smoothly continuous and gradual. However, it may be relevant that the thermodynamically open type of system of living organisms shows a Le Chatellian tendency to return to its steady-state condition when disturbed (Passynsky, 1957), so that only macroquantal changes in the genotype may be capable of displacing it to a new steady-state position (cf. Schmalhausen, 1949). Moreover, the magnitude of any change, originally in the genotype, becomes greatly increased in the phenotype. True evolutionary changes in the genotype may be equally subject to this quantum-requirement, and to the ontogenetic magnification.

Thus some mutations may be most probable events after all, and true evolutionary novelties

may occur, at present undetected, among these. It may be possible ultimately to distinguish them among the more numerous but more trivial types of mutation, and to study them experimentally. There are grounds (p. 196) for the view that these evolutionarily significant events are now relatively rare, because their prime cause, environmental long-term changes, also are becoming progressively slower or rarer, e.g. terrestrial radioactivity (Madison, 1953). Nevertheless, both remain significant phenomena and are enhanced by the fact that living organisms themselves constitute a major, and very dynamic, component of the environment: there may be an autocatalytic component in evolution.

The present thesis requires the assumption that the genetic material of living organisms is accessible to relevant environmental influences, a supposition which appeared dubious according to the Weismannian principle of inviolability of the germ-plasma. In fact, however, the induction of mutations experimentally has shown the germ plasm to be very sensitive to ionizing radiations and other agents, of possible evolutionary significance. The nucleus is now known to have a relatively rapid metabolic turnover (Hevesy, 1948), and to interact freely with the cytoplasm (Nanney, 1957). The cause of *spontaneous* genetical change is unknown: it could be environmental agents acting through the soma onto the germ-plasm, in a simple, most-probable manner. Owing to the complexity of modern living organisms, however, the ultimate manifestation may be only very indirectly related to the evoking stimulus, as already envisaged (p. 192), and the accessibility of the germ plasm therefore may be far from evident.

It is sometimes considered (Oparin, 1957, pp. 261, 351; Ycas, 1955; Blum, 1955) that orthodox natural selection had no application at the molecular level of biological systems, before they became segregated into discrete organisms. It seems meaningless to talk of the life or death of a molecule, and therefore selection also is considered to be meaningless at this level. However, according to the present view probability, selection, survival, and life all have on this level the same essential meanings as for more complex systems, though their particular manifestations may vary according to the complexity of the system. A molecule may be said to die as an entity when it decomposes into two smaller molecules or when it is "eaten," through combining with a larger molecule. More complex systems also die only as entities. A possible natural example of selection at the molecular level was pointed out by Cernovodeanu and Henri (1910): most of the types of inorganic molecule at present common on earth have photodecomposition energies outside the range of the energy-quanta actually received from the sun. This perhaps implies that other molecules may be formed just as frequently, but more frequently suffer the death of photo-decomposition.

Bornemisza (1949) boldly refers to all molecules as small living organisms, for they have a dynamic permanence; but some reservation is necessary (Pirie, 1953), since some molecules, for instance aluminum silicate, the main constituent of clay, are very inert chemically—a property very foreign to life. Such inert molecules are conspicuously absent from living organisms (Pringle, 1953), though they might have provided a physical matrix on which the first living systems condensed (Bernal, 1951). It would seem necessary to restrict the term "life" at least to the behavior of molecules which are chemically labile and are spontaneously and regularly reactive under natural terrestrial conditions. Strictly it should be reserved for particular systems of such molecules, for only collectively can such dynamic molecules have the permanence and continuity which are the minimal properties of living organisms.

It has been pointed out (Osborn, 1918; Bernal, 1951) that the stars might be considered "alive" on the criterion that they survive by natural selection, i.e., by the natural course of events. At the other extreme it is equally true of typical atoms (Haeckel, 1870; Butler quoted by Wood Jones, 1950; Oparin, 1957), and no doubt of protons and electrons. The rapid fate of such bodies as mesons and radioactive atoms only emphasizes the natural longevity of the more common bodies in this size-range. It further indicates that the stability of the latter also is dynamic, like that of living organisms, and differs essentially in being simply a physically, and not a chemically, controlled stability.

Dynamic stability is an inexplicable paradox unless some essential activity is cyclic, as manifestly it is in atoms and in stellar systems. In living organisms the counterpart must be cyclic systems of chemical reactions (p. 196). These re-

actions constitute the "metabolism" of the system and collectively they produce a permanent entity which is the "morphological" component, or the "fabric" of the system. The permanence of the latter is to some degree independent of the speed of metabolism, i.e., of the individual molecular reactions or of the turnover of reagents; indeed, the speed may be virtually zero in the state of suspended animation (Keilin, 1953).

There is thus a rather sharp and paradoxical dualism in biological systems: a variable and often vigorous metabolic flux combined with great morphological stability (Schmalhausen, 1949). The latter is an important "emergent" property at the biological level. Furthermore, it is an expression of the steady state typical of the open, biological type of system (von Bertalanffy, 1949; Oparin, 1957). Like its component metabolic subsystems (Passynsky, 1957), it is restored in the face of any moderate disturbance (Needham, 1952), chemical, physical, or mechanical. If driven beyond the limits of this restorative power an individual will die, though the species may survive through the "dynamic" selection of individuals with a "norm of reaction," a steady state, appropriate to the new conditions (Schmalhausen, 1949, p. 95). As already suggested (p. 8) the new norm of reaction may depend on quantal mutations.

THE DUALISM OF LIVING SYSTEMS

This dualism, the metabolic lability and morphological stability, has been emphasized frequently by workers in this field, usually by those who also feel that the metabolic aspect is not sufficiently stressed. Oparin (1957, p. 359) refers to the paradox of permanence with change and maintains that the fundamental organization of living matter is its organization in time, i.e., its metabolism (p. 364). The "fluctid" and "statid" antithesis of Mitchell (1957) is specifically focused on this paradox. Bose (1953) defines an organism as an "autonomous unit through which there is a continuous flow of energy." Bornemisza (1949) described the individual as the carrier or vehicle of recurrent changes and again, even more strongly, as nothing more than a phase of transition for the free energy of the environment. He even goes so far as to suggest that, directly or indirectly, metabolism is the real "aim of nature." Perhaps this is an overemphasis (p. 201), but most writers no doubt have erred to the other extreme, sometimes overlooking the metabolic aspect completely.

The paradox is not often accommodated in theories on the origination of life because it is not easily resolved at that level. There is a tendency to overlook the issue, through considering only the tangible, stable morphological entity; this is clearly visualized and it has been neatly transmitted, down the later generations of evolution at least, by a precise genetic mechanism. If it is believed that the latter can produce, as typical mutations, all the "material" required for evolutionary change (p. 192), then there might seem to be nothing further to consider. By extrapolating backwards, these genetic mutations of cellular organisms are often believed to derive directly from simple chemical changes in the precellular stage of the "pristine biological soup" (Haldane) or "metabolizing ocean" (p. 191). At this level it is difficult to visualize any counterpart of the morphological entity, or the evident dualism of later stages, so that there is the danger that the long-term chemical changes, of evolutionary significance, may not be distinguished from actual metabolic reactions; it is then easy to fall into the error of assuming, at will, that either or both aspects have been considered.

There is the further complication, of course, that the morphological entity or fabric also undergoes an ontogenetic developmental change in each generation, and this even more easily might be confused with metabolic activity, particularly at the pristine chemical level, for its rate is much nearer to that of metabolic reactions than is that of phylogenetic change. However, since in broad outline ontogenesis recaptulates phylogenesis (cf. J. Needham, 1930; Wald, 1952), any confusion of metabolic with ontogenetic change (ontobiogenesis) is easily extended to phylogenetic change (phylobiogenesis). Three significant time-scales therefore must be distinguished, that of metabolic reactions in the living body, that of the genetic ontogenetic time-scale, with the individual generation-time as unit, and that of the evolutionary time-scale measured in geological units of time.

A common tendency, therefore, is to postulate what in effect are metabolic changes but on the genetic-phylogenetic time-scales. There re-

sults the implicit or explicit postulate of a living system performing a reaction at one geological age and then having to wait for a further age before performing the next step in that reaction-sequence. It seems legitimate to overemphasize the kind of error to this extent, since the actual rapidity and amount of chemical activity, i.e. of metabolism, probably always has been very great, and because this is so commonly overlooked. Indeed, a number of writers have explicitly suggested that the rate of metabolism was very slow in the early stages (Gaffron, 1957), and others have implied this. On further consideration, some confusion of metabolic with evolutionary rates would perhaps be admitted by these authors.

It is invidious to single out any particular example. The following may be quoted without documentation: "... with the passage of time molecules of the organic compound would tend to accumulate near the site of formation. . . ." Here, clearly, the evolutionary time-scale is envisaged. The molecules are attributed a sluggishness which would be inconceivable to the chemist: an excited atom unloads its energy in 10^{-8} seconds (Jones, 1950). This error in rate-scale is encouraged to some extent by the consideration that biocatalysts at first very probably were much less efficient than those familiar today, and metabolism correspondingly slower (Gaffron, 1957), but it scarcely needs emphasis that the slowest of chemical reactions is incomparably more rapid than the fastest of evolutionary changes. Moreover, even the calmest and most inert of environments would scarcely give the pool of molecules such infinite leisure to accumulate at the site of formation! Here there is a common misconception that this material would not decay because there were no (modern) organisms of decay (Hardin, 1950). In fact, no doubt, catabolism was as active, relative to anabolism, as it is today; and if this be admitted, then it becomes impossible to ignore metabolism as a major factor, even at its slowest imaginable speed.

There is little reason to doubt that in fact metabolism had always a *relatively* high order of activity, and this might be termed the "metabolic," as contrasted with an "ametabolic" theory, which sees little significance in metabolism during the origination. Several corollaries follow, of which perhaps the most important is that such rapid and world-wide activity would leave few possible metabolic pathways untested for "viability," and indeed few not fully exploited from a very early stage. Most would be "tested" with a frequency which could only be expressed in astronomical figures. Moreover, this would probably include all possible "recombinations," i.e., new chemical encounters due to spatial, and other, types of reshuffling. It must be remembered that the main raw materials for all conceivable biological substances—simple compounds of carbon, hydrogen, oxygen, nitrogen, sodium, potassium, and calcium—were present and panglobally distributed from the outset. The only serious problems could have been those of an adequate synthesizing machinery (p. 203) and of the appropriate spatial and temporal coincidences of the materials (p. 205).

The less common biological materials, sulfur, phosphorus, iron, iodine, etc., conceivably, but by no means certainly (Gaffron, 1957), may have required some time to reach their equilibrium with living systems. For instance, there may have been no positive demand for a particular element until one had been created through its fortuitous reaction with components of the basic fabric. Nevertheless, the probability is that, at the prevailing metabolic rates, even this would be on the historical rather than the geological time-scale.

If these contentions are correct it should follow, as a second corollary, that all organisms contain very similar materials. This in fact is true (Gaffron, 1957), if the more specialized, taxonomic differences in proteins be excepted (p. 198). There are some other exceptions, but these tend to prove the general rule. Thus some plants have much selenium in place of sulfur, and some coals contain germanium (Pirie, 1957; Synge, 1957), though the latter conceivably may be a posthumous enrichment (Hallam and Payne, 1958). Again, animals use a variety of metal cations for respiratory purposes, and a variety of phosphagens (Baldwin, 1953), and some molluscs have galactogen in place of the common glycogen. Even so, the variety is conspicuously limited, in view of the maximal variety theoretically possible (Gaffron, 1957).

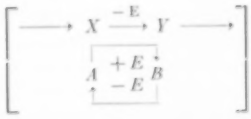
Moreover, the general tendency during evolution has been to reduce the number and variety of biochemical substances (Lwoff, 1944; Berg-

mann, McLean, and Lester, 1943; Shorland, 1952; Florkin, 1957). Primitive organisms tend to synthesize a whole series of related compounds, whereas those more highly evolved have come to exploit only one or a few optimal members of the series. Proteins may be rather exceptional in this respect, since it is usual to find a whole family of related molecules still synthesized by even the more highly evolved of extant organisms (Fox and Foster, 1957). This may be one main reason why the taxonomic specificity of proteins is so great, and probably ever increasing (Pirie, 1957b). Related species differ in the spectrum of molecules which they synthesize in any particular class, and in the "modal" member of this spectrum. This kind of specialization, and the variety it produced, would be impossible before discrete, relatively isolated living systems had evolved and given rise to distinct local environments. In the initial panglobal soup, all syntheses then possible were widely, if not absolutely uniformly, distributed. No doubt morphological specificities depend mainly on protein specificities.

It follows as a third corollary that after a very early stage of evolution the probability of real chemical novelty would be relatively low and that in general any change would be of the type familiar in more recent evolution, essentially small and morphological. Continuing preceding arguments, the only circumstance which could permit true evolutionary novelty at the basic chemical level would be a completely new, previously unexperienced change in environmental conditions. In fact, a progressive, slow climatic change is generally believed to have characterized the earth's history, and the progressive evolution of organisms themselves would add to the general fund of environmental novelty (p. 193). Fundamental evolution of this type therefore has continued to the present day, if at a diminishing rate. World-wide climatic changes would affect all species of organisms simultaneously, though their individual responses might vary. The changes would be so slow as to be irrelevant for the progress of a particular metabolic process from day to day, or even from year to year, and would be significant only in so far as they affected the permanent fabric (p. 193) carrying out the metabolism. Thus, over the years, one type of reaction might grow progressively less, and another more, probable and the "turnover" via one pathway would change relative to that via another path. Although panglobal in action the climatic change would affect individuals and species according to their different genotypes, and there would appear plenty of variety on which recombination and natural selection could further act. A very probable example of a significant long-term trend is the gradual change in the oxidation-reduction level of the terrestrial media (its atmosphere and waters) due to the progressive loss of hydrogen to outer space (Urey, 1952). As the level changed, presumably metabolic reaction-sequences proceeded to a progressively different stage. It is not necessary to assume that the rate of this, or of any other, change was necessarily significant throughout the entire history of the earth, and today there may remain very little of this basic chemical evolution, exploiting reactions previously improbable.

Here, just as much as in the later, more familiar morphological type of evolution, it is the fabric of the system which acquires the novelty; and the role of the metabolic component is not immediately obvious, though it has been recognized (p. 193) that the fabric owes its permanence and indeed its very existence as a system to the cyclic components of metabolism. In concluding this section it may help to try to visualize the precise relationship between the two components in simple pristine systems. The simplest possible system is

where A and B are molecules of a simple fabric. In modern autotrophs the energy necessary to drive this simple cycle might be direct solar energy, but most metabolic cycles of all organisms it is provided by the oxidation of stored solar energy, in the form of a suitable substrate-molecule X:



The step $X \longrightarrow Y$ may be one link in a longer series of reactions, as indicated, leading ulti-

mately to such products as CO_2 and water—a one-way process for these materials, in this system. *AB* is the only cyclic component, for the energy released in the restoration of *B* to *A* passes to some useful, but also irreversible, work-process. Clearly *AB* is a permanent functioning system whatever the speed of its cycling (p. 193), but it seems an excellent vindication of the familiar French paradox "plus ca change, plus c'est le meme chose." Unlike evolutionary change it is essentially reiterative (p. 192). A simple evolutionary change would be the cyclic conversion of *A* to a derivative C instead of *B*, due to a change in environmental conditions.

It is important to note that *AB* has essentially the properties of a catalyst or enzyme, since in theory it is recoverable quantitatively after an indefinite amount of activity. It conforms, in its simple way, with the picture (Dixon, 1951) of enzyme-systems as systems of labile catalyst maintained in being by the occurrence of the reactions they catalyse. It is possible to understand the conviction of Alexander (1948) that enzymes provide the "general solution for all biological enigmas," including the origination of life. All or virtually all enzymes are proteins and most proteins are enzymes, including even such proteins as myosin, the primary properties of which are mechanomotor (p. 206).

The permanence of the enzyme-fabric is not so absolute as the above diagram indicates. There is a phenomenon of wear and tear among enzyme molecules for which in some cases quantitative estimates are available (Haldane, 1954b; Krebs and Kornberg, 1957; Comfort, 1956, p. 164). It is very heavy wear. Studies using isotope-labeled materials indicate that such wear and tear may (but see Cohn, 1956) be offset by a metabolic exchange process in which the damaged molecule, or part, is replaced by its identity in a reserve "pool" of metabolites. A precise replacement of this kind, molecule for molecule, may not have been biologically necessary until discrete segregated units, or organisms, had evolved, though of course a net, statistical replacement would always be necessary, in any system which was to "survive." Metabolic exchange is probably related also to growth (p. 206), which could be regarded as the parallel phenomenon at a higher level of magnitude, to replace wear-and-tear losses, by death, among whole organisms. Reciprocally, wear-and-tear losses among fabric molecules may be regarded as a genuine "death"-phenomenon at that level, and it is important to note how it differs from the simpler analogy given on p. 193: to the enzyme molecule its normal chemical reaction is life, not death.

Inorganic catalysts also suffer some degree of loss by wear and tear, which therefore must be regarded as a most probable phenomenon. The making good of these losses consequently can be a most probable process only under some additional condition which is peculiar to living systems, and gives them their unique property of self-perpetuation. No doubt this is a critical point in connection with the basic axiom (p. 190). It could be a most probable process only in an open system.

The rate of metabolic exchange bears a relationship to the general rate of metabolism of an organ or organism, and this may be taken as a further demonstration that the maintenance of the fabric is closely dependent on metabolic activity (Oparin, 1957). It maintains the "form" or tangible manifestation of the living organism in rather the same unobtrusive way as the form of a river is maintained by the water ceaselessly flowing through it. Though unobstrusive in the evolutionary landscape, however, the metabolic stream must not be ignored.

FURTHER ERRORS OF THE "AMETABOLIC" VIEW

From errors of time-scale (p. 194) it is easy to slip into other errors of scale, e.g., from supposing that the acquisition of one new species of molecule might require a geological age to the implication that a single molecule would be adequate to establish that molecular species—as it might be in the happy but very improbable event (p. 198) of possessing immediately the biological property of self-duplication. Again, specific examples are invidious selections from those available and may be quoted without reference: "... therefore with the attainment of a molecule of adenosine monophosphate, many compounds of carbon became available to the primitive organism ..." and "... if a protein combined with one or more molecules of a polydesoxyribonucleotide, this compound could satisfy the requirements for the original living organism. ..." In some instances "a molecule" may be used figuratively to mean the species of molecule, but this is far from certain in such quotations

as the first, above. In any case the usage is dangerous because inadvertently, at least, the shift to a literal conception is so easy.

Objections to the idea that at any stage the acquisition of a unique molecule might be a significant event include those already raised (p. 191) against the view that the origin of life could have been a unique event. On the present view (p. 195) that all essential metabolites are substances which, from the earliest stages, were available in quantity, the probability of any unique molecule of a new type acquiring immediate significance and incorporation seems very small.

This is not to deny molecular evolution and novelty. It seems likely that the taxonomic, i.e., genetic, specificity of proteins, nucleic acids, and some other materials has increased progressively (Pirie, 1957b), even during recent evolution (p. 196), and this conceivably might involve the evolution of unique new species of molecules. In all cases, however, this must be an orderly and continuous evolution from a nearly related precursor, by a process which in effect is so probable that it can and will recur with indefinite frequency. In the spirit of the present issue it would never be a unique event. Moreover, in all probability, even this type of novelty could become a unique property of *particular* living systems only after these had evolved into discrete organisms, isolated from the rest of the biomass (p. 196). It would be common property in the early, most critical stages of evolution.

While this would apply to all the biologically significant carbon compounds, it remains possible that certain inorganic molecules might be uniquely acquired and subsequently exploited, although reasons have already been given (p. 195) for believing this to be improbable after a very early stage. Supposing that a unique molecule of such a completely new, unexperienced substance, *S*, could be presented to a living organism, what are the possibilities of its permanent incorporation, in significant amounts? A molecule of the majority of possible compounds in this category would inevitably be irrelevant or useless to metabolism, if not frankly deleterious, and would be swept passively through the body, or detoxicated and excreted. In the event of its reacting with one of the more trivial constituents of the body, such as a simple substrate, then at the most a single molecular reaction would be influenced before it suffered the same inevitable fate of excretion. Reaction with a molecule of the fabric could lead to its retention in the body, as part of a modified enzyme-molecule, and in some cases this might have novel and useful properties in consequence. Given also a mechanism for genetic transmission, therefore, it might be preserved in the species, as well as in the initial individual.

However, the die is very heavily loaded against a permanent incorporation in significant amount. As a molecule of a non-carbon compound, *S* itself could not be replicated to offset its wear and tear, except by the incorporation of more molecules of the same type, or of the components of these, but this renders meaningless the initial postulate that there could be a significant acquisition of a unique molecule. Any material sufficiently abundant to meet the requirement for an assimilation-rate adequate for the replacement of losses by wear and tear must already have been fully exploited in the past.

It is clear that to be incorporated as a significant novelty a new material must not only be available in quantity but must be assured a mechanism of assimilation and propagation. It therefore must be a spontaneous, most probable incorporation into the fabric of the system—the component which possesses the power of self-propagation. This helps to explain why it is often possible to consider only the fabric and safely ignore metabolism, when dealing with certain aspects of the problem of the origination of life. In such aspects as those now to be considered, however, this would be very dangerous.

Probably the most important error in the "ametabolic" conception of pristine life is the contention (Oparin, 1957) that solar energy was not necessary in the early stages. Its importance today is unquestioned, and if, as seems certain (p. 195), there always was a high rate of metabolic activity, then solar energy must have been equally essential at all times, for there is no other adequate source of energy. It has sometimes been supposed that long-term climatic changes on earth could have been an adequate source, but this involves the gross error of rate-scale already discussed. Another suggestion, that the first organisms lived on a store of organic materials already present (Wald, 1952; Lanham, 1952), again neglects the time-scale problem and

confuses metabolic with evolutionary events. Blum (1955) has computed that on a most sanguine estimate such supplies could last only a few thousand years. In any event, if the origination was panglobal this organic material was probably part of the living system—from the outset. To call this "abiogenetically" produced material (Hardin, 1950) begs a fundamental question.

SOLAR ENERGY AND THE ORIGINATION OF LIFE

Those who in this way deny that solar energy was at first critically necessary usually also contend that photosynthesis began only much later and more generally, that the first organisms were not autotrophs but heterotrophs (Horowitz, 1945; Hardin, 1950; Oparin, 1957; Braunstein, 1957). The further, or perhaps antecedent, hypothesis is that initially reducing conditions prevailed (Urey, 1952; Oparin, 1957), thus proving that there was no photosynthesis or any other endergonic (energy-storing) reaction releasing free oxygen. There is evidence that free oxygen in fact was not present in the atmosphere until 800 million years ago (Urey, 1952), but for the earlier stages there are other possible endergonic reactions which could meet the indispensable requirement that there can be no life without metabolism, and without a regular, high rate of "fixation" of solar energy, visible or ultraviolet. It may be doubted if this was any more spontaneous (probable) than it is today (Hardin, 1950) or that it was "abiogenetic" in consequence. On the present thesis, biogenetic processes also are most probable (p. 190). Then, as now, heterotrophs could exist only by the grace of autotrophs. Whether or no there was free oxygen in the atmosphere, conditions scarcely can have been generally reducing, in the presence of such vast amounts of water, silicates, and other oxidized compounds. Conditions may have been, and probably were, less oxidizing than today, owing to a progressive loss of hydrogen from the atmosphere—a process which has given Venus and Mars likewise an oxidizing atmosphere. Calcium carbonate has become increasingly abundant in more recent sedimentary rocks and this indicates an increase in particular oxides, and in alkalinity, consistent with the loss of hydrogen. However, the majority of the cooler stars also may have oxidizing atmospheres (Russell, 1941), so that highly reducing conditions in the early biotic stages of the planet earth seem far from certain.

In any case, the existence of free oxygen, or of any particular level of oxidation/reduction, is less important biologically than the ability to establish and maintain an energy-potential between organism and environment. This is the motive force by which the living system is able to "do work on" its environment. Under present-day conditions it depends on the building up of relatively reduced compounds, against a more oxidized, and therefore oxidizing, atmosphere. In theory other types of endergonic reaction, not involving reduction, could have been possible at one time, but there is no evident candidate for this; for, as Spiegelman and Sussman (1952) have pointed out, every reaction is in effect both an oxidation and a reduction. The general redox level well may have been at first more reducing than now, and the necessary energy-potential may have been achieved without the release of free oxygen. The change in general level has probably been one of the major irreversible climatic changes (p. 196). It might be added that if environmental conditions in fact had been generally reducing at first then the relatively reduced organic materials existing at that time could have possessed little or no energy-potential relative to their environment.

The ability to promote the synthesis of many biologically important materials such as amino acids and dicarboxylic acids, by the use of ultraviolet (Baly et al., 1935; Calvin et al., 1956) and other ionizing radiations (Garrison et al., 1951; Paschke et al., 1957; Hasselstrom, Henry, and Murr, 1957) and of other forms of energy directly or indirectly derived from the sun, e.g. electrical discharge (Miller, 1957; Abelson, 1957; Oparin, 1957; Badahur, 1957; Terenin, 1957) and heat (Fox et al., 1956), seems to be relatively independent (Abelson, 1957) of the particular state of oxidation of the raw materials used. Some have claimed best results, experimentally, with relatively reduced materials but Pavlovskaya and Passynska (1957) obtained the better yield of amino acids and other metabolites using relatively oxidized materials. Living plants today, using visible light, since relatively little ultraviolet radiation now penetrates the atmosphere, are able to utilize such highly oxidized materials as CO_2, H_2O, and NO_3, to synthesize all important metabolites, both fabric and sub-

strate. It is also noteworthy that porphyrin catalysts are able to catalyse both oxidations and reductions of organic materials (Krasnovsky, 1957). Under the conditions which lead to coal-formation the organic material loses both oxygen and hydrogen, conceivably by concurrent oxidation and reduction processes. The inference seems to be that the particular level of oxidation in the environment is not critical and that organic materials are so labile that virtually none of their potential reactions are highly improbable on earth, now or in the past.

The only possible alternative to solar energy is the heat energy leaking out from the interior of the earth. This is calculated to be 2.5×10^{17} kcal per year, compared with 0.66×10^{21} kcal per year for solar energy absorbed by atmosphere and earth (Oparin, 1957, pp. 161, 165), i.e. about 0.04% of the latter. It is a considerably smaller fraction of the total solar energy striking the earth, for only a small fraction of this is absorbed. It cannot be asumed, either, that all of the terrestrial heat could be utilized, so that at best this must be a very minor source of energy. In any case it is the light, rather than the heat energy of the sun which at present is utilized for the critical endergonic reactions.

THE CYCLE OF ORGANIC MATERIALS

The rapidity (p. 197) with which available organic material would be exhausted in the absence of continuous new synthesis indicates that it must be circulating repeatedly (Blum, 1957). That it does so is common knowledge, particularly to the student of agriculture. In addition to the organic cycle as a whole, cycles for the individual elements carbon, oxygen, nitrogen, etc., have been traced. It is estimated (Wald, 1954; Pirie, 1957) that an oxygen or a carbon atom circulates in an average half-period of some hundreds of years, a value of the same general order as Blum's exhaustion-time, with an adequate margin of safety; material therefore is circulating at about the optimal speed. Anything much faster or slower would adversely affect the essential continuity of the process (p. 202). The cycle has a time-scale not very different from the longest known generation-times, i.e., it conforms to the intermediate biological time-scale (p. 194).

The rate is noteworthy also because it rules out any possibility that the familiar diurnal and seasonal cycles of influx of solar energy could be directly reflected in the organic cycle. The completion of the latter is due simply to the fact that energy acquired from the sun tends to dissipate again, thereby returning the material to its "starting point." The cycle is a spontaneous, most probable process, but whereas the uptake of energy is closely geared to solar energy-influx, dissipation is relatively independent of the original source. Living organisms can retain acquired energy for considerable periods: for instance, all living species must be able to survive night and winter, and do so, if necessary by "suspending animation" (p. 193). Variations in day-length and insolation between tropics and poles do have some, but relatively little, significant effect on living systems; thus the color of some arctic plants changes with the season, the change possibly being similar to that in the presumed vegetation of Mars (anon., 1954). The diurnal "cycle" of solar energy more correctly is an intermittent influx, while dissipation continues day and night.

These considerations somewhat weaken a very stimulating theory of Kavanau (1945, 1947) concerning the role of the diurnal energy-flux in the origination of life. His thesis is that the oscillatory flux—net influx by day and efflux by night—would drive pristine chemical systems to and fro between more and less endergonic conditions, in which process the more stable, better buffered systems would "survive," in a mid-way energy-condition, and would evolve as living organisms, while the less stable would be eliminated. The nature of this stability needs careful examination since, on the present thesis, living systems are just those most dynamically sensitive to the energy-flux: the morphological and genetic stability they achieve (p. 193) is essentially of the dynamic, steady-state type. It is not clear that Kavanau intended this kind of stability rather than a condition of maximal inertia, i.e., essentially a static stability, but in any case it is not certain (1) that systems which are very sensitive to the energy-flux are eliminated, or (2) that the diurnal "cycle" is capable of producing the desired end-result, that of dynamically stable living systems. It is difficult to believe that the nocturnal efflux could be an effective driving force: certainly it must be infinitely small and undirected, by comparison with the full force of the sun by day. An influx of energy is indispen-

sable, but formally any pattern of influx, within reason, would suffice. Pringle (1953) attributes no great significance to the diurnal solar cycle.

Granted an adequate pattern of energy-influx there is, in theory at least, considerable latitude in the way it might be used, and in the type of biological cycle resulting. Some might differ greatly from that actually observed. Living organisms might be all autotrophic, so that the whole living system was driven very near to its endergonic limit of synthesis and energy-storage and was held there in a steady-state, further influx being limited by the rather slow rate of spontaneous dissipation. This would be a far less rapidly dynamic system than that which actually exists, its circulation of material of course being as restricted as its energy-flow. This is perhaps the kind of situation visualized in some of the "ametabolic" theories, though in fact even this system could be infinitely more active metabolically than those usually postulated by that school of thought.

In passing, it is worth stressing that in fact there are only two courses open to a molecule, or a system, subjected to a unidirectional influx of solar energy. Either it will be driven to some stable "end"-condition, which in most cases will be reached immediately, and in all cases relatively soon on the evolutionary time-scale, or it will participate in a cyclic set of processes, the only type of activity which can continue indefinitely. These alternatives correspond to those followed by the inanimate and the living systems respectively (p. 194). On this and on other considerations (p. 195), it seems probable that only a single pristine living system would be formed, since if any component were cyclically perpetual only in isolation, then any chance-interaction with some other component would tend to destroy its normal ability to cycle. In any case, all possible living systems must use the common fund of carbon compounds and in this sense are potentially all parts of a single major system. At the same time there are numerous cyclic phenomena (p. 196) within the main system, more local in space and time (cf. Ycas, 1955). Metabolism depends mainly on such cycles.

The actual situation in the organic cycle on earth is that heterotrophs, often working in long series, or food chains, accelerate (1) the "use" of solar energy trapped initially by autotrophs, and (2) the consequent return of materials to their starting point. This results in a much higher order of cyclic activity than would be possible in a purely autotrophic system, and therefore it is tempting to agree with Bornemisza (p. 194) that this speed, whether in the main cycle or in metabolic cycles, is the essence of life, i.e., that turnover activity is the "aim" of life. The possibility that a faster circulation might lead to a more rapid evolution also should be envisaged; there should be more frequent exposure to all environmental variables. It may be on account of a progressive speeding in the cycle that evolution, of the taxonomic type, has continued so extensively, notwithstanding the limitation to basic biochemical evolution (p. 196). A speeding of the cycle need not involve any change in the total amount of material involved —the biomass—which probably is limited, but it will increase the amount of solar energy trapped per unit time, and this almost certainly must itself affect evolution. It is a good example of an evolutionary trend due to living organisms themselves (p. 193).

An accelerated "return" of materials by heterotrophs permits in turn a more rapid autotrophic activity, and therefore today it is almost as true to say that autotrophs exist by the grace of heterotrophs as vice versa (p. 199). However, it could be contended, on the other hand, that autotrophs now merely have to "run faster to stay in the same place." This is a matter of philosophical taste: the fact of biological importance is that there must necessarily be automatic "selection" in favor of speed of cycling in both nutritive phases, since each indirectly accelerates the further supply of food for itself. This in fact is a natural example of the paradox of eating the cake yet having it still! It is no doubt significant that certain oriental philosophies have attributed such importance to cyclic phenomena! It is also interesting to consider that this synergistic interaction between autotrophs and heterotrophs is often viewed as a competition, and rightly so, since the latter prey on the former. The precise nature of competition in each case needs careful examination, therefore. It seems likely that competition in general, e.g., also that between two heterotrophs, will speed the cycle. Pure optical isomers have probably been selected for the same reason (Wald, 1957).

The spontaneous decay of many biological materials in the absence of all heterotrophs,

including microorganisms, seems to be very slow (p. 203), and under present conditions, certainly, heterotrophs are essential for continued life. The real problem for living systems therefore may not be, as often supposed, the synthesis of endergonic materials (p. 202) but rather their subsequent destruction.

Since it would seem that selection should tend to speed up all phases of the organic cycle indefinitely, it is interesting to consider what factors have limited the actual rate of turnover. The present rate may be far above the minimal value necessary to maintain a continuous circulation worthy of the term "life," but on the other hand it is almost certainly slower than the theoretical limit set by the availability of solar energy. It is calculated from the data of Transeau (1926) that at the present day only 0.12% of the solar energy reaching the surface of the earth is utilized by living organisms. There may be insufficient material to use more than a fraction of the available energy, even under otherwise optimal conditions, and certainly the spatial distribution of the material may be a limiting factor. Again, at any particular place or time, energy must be available in quanta of the correct value for the required reaction. No doubt there are also other contingent requirements. There may be "inherent" limitations in the organic cycle itself—in its ability to return materials "to base." A heterotroph must find and capture its food, and the microorganisms of decay lead a precarious life, with long periods of suspended animation. It might be possible to calculate the rate of turnover to be expected in virtue of the different possible limiting factors and so to decide which are actually relevant.

A uniform rate of circulation is probably more important, biologically, than mere rapidity, and this is perhaps one of the factors limiting the over-all rate. Elton (1935) has pointed out the calamity which could result from the excessive multiplication of any one species of organism. It might eat out its food-supply and then itself die of starvation, for the half-starved individual is a liability to the species. In turn, this species would then be unable to play its normal part in the whole organic cycle, which might be seriously affected in consequence. Perhaps few, if any, single species could be so critical for the whole cycle, but conceivably there could be a more general disturbance of the autotroph/heterotroph ratio, and this could be disastrous—to both nutritive types, ultimately. Fortunately, there is a good deal of LeChatellian autoregulation, i.e., negative feedback, which helps to maintain steady population-sizes in the wild.

The "stockpiling" of particular materials is equally deleterious. A considerable amount of coal, peat, guano, chalk, and probably oil and other materials (e.g. pitch) has accumulated, in fact, through unbalanced activity of particular living systems. There is estimated to be two hundred times as much carbon locked up in coal and carbonates as in the CO_2 of the atmosphere and oceans (Pirie, 1953) and, while the system probably could endure even more of this freezing of assets, no doubt there is a limit, and already it must have affected evolution and life. Here again, autoregulation in the system as a whole is probable, thermodynamically, for the accumulations are mostly of highly endergonic materials, which should oxidize spontaneously (p. 203). The rate can be slow, however, and a premium is placed on uniform cycling. Stockpiling and bottlenecks are equally disastrous for the cyclic processes of metabolism in the individual organism (p. 196).

The Energy-Problem

From a number of indications above it seems probable that available materials, rather than energy, always may have been the major factor limiting vital activity. It seems certain, for instance, that plants today could synthesize more living substance if they had more CO_2 available. It is usual to suppose that living organisms are material systems competing for energy, whereas it seems more likely that they are energy-systems competing for materials. In such a situation chemical condensations, syntheses, and other endergonic reactions might be expected to proceed spontaneously as a "sink" for excess solar energy, and organic molecules would spontaneously unite with, or "ingest," other molecules; i.e., competition is a most probable phenomenon. At a higher order of magnitude, again, the formation of coacervates (p. 204), the unilateral ingestion of one organism by another, and the mutual fusion of two individuals in an act of conjugation may become spontaneous under the same conditions, although at this level the gain in free energy to the *combined* system may be

relatively small (p. 203). Nevertheless, this may have been the spontaneous motive-power for primitive heterotrophy, predation, and sexual conjugation.

With a possible abundance of energy for activating molecules and a shortage of material, endergonic processes well might be expected to continue to very high levels in such material as was available, so that relatively complex substances may have been synthesized from a very early date (cf. Lanham, 1952), even if less rapidly than today. The advent of heterotrophs, by tilting the metabolic balance in favor of catabolism, presumably was a most probable process, since it would further increase the outlet for the stored energy, and the return of materials to base.

This argument leads inexorably to the conclusion that the energy-supply has never been a serious problem for life, and it is important, at this stage, to examine the facts which have seemed to indicate that it is a serious problem. In particular, there is the apparently high "improbability" of syntheses such as that of the peptide bond (Lanham, 1952). For the first step in protein synthesis, the formation of a dipeptide, the equilibrium under average terrestrial conditions of temperature, etc., lies 99 per cent in favor of the free amino acids (Borsook, 1953; Blum, 1955). This low probability of synthesis, however, applies only in the complete absence of an external supply of energy. The actual problem, for living organisms, therefore has been purely one of effective manipulation of the abundant energy available. It seems likely that energy of the correct quantal value was always available, as it is today, in the presence of natural catalysts. Virtually all inorganic substances can catalyse a variety of organic reactions (Oparin, 1938, p. 163) and the simple homologues of such key enzymes as the porphyrin-protein systems have already a small, but positive, catalytic power of the same kind (Calvin, 1957; Nicolaev, 1957). Such substances therefore may have been the actual prototypes of the enzymes in question. It is thought possible, moreover, that catalytic molecules as complex even as the porphyrin may form spontaneously (Gaffron, 1957; Oparin, 1957, p. 202). Organic reactions in general require little energy by comparison with laboratory reactions, and the average intermolecular bond-energy requirement is not much greater than that of the hydrogen bond (Bernal, 1951, pp. 13, 45). Simple condensation reactions, such as that of acetylene to form benzene, are exothermic, and aliphatic molecules of progressively longer chain-length are believed to have formed spontaneously as the earth cooled (Oparin, 1957). Industrial polymers are best initiated at high temperature, but low temperature favors the increase in length of their chains. A number of reduction reactions also are exothermic (Urey, 1952, p. 358). A good deal of endergonic synthesis therefore may have been coupled with exergonic syntheses, the complete system proceeding spontaneously.

The pristine autotrophs possibly had available considerable amounts of ultraviolet energy, of high quantal value, and must have performed syntheses at least as readily as modern autotrophs, using mainly light energy of lower quantal value (p. 200). The latter illustrate an important type of evolution, for they are now hypersensitive to energy of the higher quantal values (Cernovodeanu and Henri, 1910). It induces mutations (p. 192) and can have very deleterious effects.

The energy requirement for successive peptide bonds in the synthesis of a polypeptide becomes progressively less (Borsook, 1953) and therefore may be insignificant when incorporation is into large protein molecules. Living organisms always assimilate into cells already containing abundant protein, and further synthesis by incorporation into this may be virtually spontaneous. It is clear that formed proteins should have a great advantage over isolated amino acids and oligopeptides, as "nuclei" for further peptide synthesis. Moreover, these alternative nuclei themselves might be "mopped up" or "devoured," by the larger protein molecules, and for these reasons, no doubt, the origin de novo of viable new organisms today is rather highly improbable. It is also worth reemphasis that existing proteins may act as templates in this non-specific manner as well as, perhaps, in the specific way often postulated, by controlling the actual sequence of amino acids in the new peptide (p. 206).

If the endergonic synthesis of biological materials from simpler organic compounds were in fact very improbable, then reciprocally it would be expected that any which were formed would decompose spontaneously, particularly on reducing the external energy-level, e.g., by cooling.

There is in fact evidence that some proteins do show some tendency to break up into smaller fragments, and to do so somewhat more rapidly at low temperatures (Petermann, 1949); but in general the stability of biological materials is surprisingly high (Keilin, 1953; Haldane, 1957; Abelson, 1957). A carcass free of microorganisms does not autolyse spontaneously, but essentially by the action of specific enzymes, present in all tissues. The main function of these intracellular proteases is believed to be in the proteolytic direction (Jensen, Lehmann and Weber, 1956) in which case presumably proteolysis is far from spontaneous in vivo. In fact, it needs as much respiratory energy as virtually any metabolic process in the body (Simpson, 1951). Biological material does not decay rapidly in polar climates, but much of it is stable virtually indefinitely, as shown by cold-storage practice and by the perfect preservation of mammoth carcasses in Siberia. It is also stable in the absence of light (perhaps more stable), and in the presence of abundant atmospheric oxygen, unless heated strongly (e.g. coal, oil, etc.) Clearly a high activation energy is required to initiate degradation, and the molecule of material rests, as it were, in an energy trough from which it is virtually as easy to displace it into the level of the next endergonic, as of the next exergonic, stage. Thus ionizing radiations promote both degradation, and synthesis reactions (Garrison, 1951). Equilibrium favors the polymerization of diphosphonucleotides rather than the reverse reaction (Blum, 1957; Warner, 1957) and protein tends to unite with nucleic acid spontaneously (Fraenkel-Conrat and Williams, 1955). Under simple in vitro conditions soluble collagen readily polymerizes into fibres virtually identical with those formed in vivo (Wald, 1954).

Moreover, because of the decreasing bond-energy requirement with increasing molecular size, the stability probably *increases* with the complexity of the material (Wald, 1954), contrary to what might be expected. Most conjugated proteins in fact are more stable and less reactive than their separate components (Deborin, 1957). Stability is inherent in the properties of carbon (Edsall and Wyman, 1958).

THE ORIGINATION OF DISCRETE ORGANISMS

The above considerations in general are valid whether or not the living systems are organized into discrete organisms, but no doubt a condensation and segregation of the material has considerable selective advantage, particularly in virtue of its increased morphological-genetic stability (p. 194). Aggregates do in fact form spontaneously under appropriate conditions. In the laboratory (De Jong, 1935, 1949; Booij and De Jong, 1957; Oparin, 1957) such bodies or 'coacervates' form spontaneously in mixtures of solutions of two or more colloids, of which one at least is a protein. On mixing the clear colloidal solutions, at a *p*H intermediate between their respective isoelectric points, fluid droplets of the size-range of small organisms form instantaneously. They have a distinct limiting membrane, are relatively stable, and show many biologically interesting properties, such as the abilities to form vacuoles and other additional phases inside, to undergo sol-gel transformations, to form orientated crystalline and liquid-crystalline structures, to absorb materials selectively from high dilutions outside, and to grow. Starting in this way, with large amounts of selected colloids, it is very easy to produce spontaneous coacervation. The natural counterpart may have been formed more gradually and sporadically, but nonetheless spontaneously and inevitably.

Coacervates are stable because they represent an equilibrium state, a further example (p. 204) of increasing size and complexity leading to greater, not less, stability. The aggregation tendency is particularly striking here because of the resulting stability, assisted by the definite limiting membrane, but it is also a very general property of all matter, living and inanimate. Apart from the purely non-specific attractions of gravity, van der Waal's forces, etc., there are more specific subchemical attractions such as hydrogen bonds. The latter largely govern the behavior of water and of all aqueous systems, including living organisms therefore. Specific, homogeneous aggregations are a commonplace of geological metamorphosis, and crystals are known to purify themselves, after initial formation in the presence of contaminants (Alexander, 1948, p. 61). These phenomena occur even in solid or semisolid media, and clearly there is much more scope for such aggregations in the fluid media of biological systems. Practically all of the protein in a solution of 10^{-5} concentration accumulates in the surface layer within 24 hours

(Höber, 1947, p. 196). These local aggregations of pure materials may be regarded as anti-entropic. There is probably a strong tendency to aggregation by biological materials (Wald, 1954) at all levels of magnitude, for this and for other reasons (p. 202), discrete coacervate-like bodies appearing at a critical size. In the synthesis of proteins and in the mating of gametes, some degree of hetero-aggregation seems "preferred." There are natural selective advantages in this, but it may be a most probable tendency, also, just as mixed crystals are formed with less energy expenditure than pure crystals.

A widespread tendency towards specific aggregations is important since it removes any serious problems of spatial dilution and inaccessibility, particularly if there is also energy in the system to assist overt movement of molecules (p. 206). Calculations have indicated an organic content of the primeval sea as high as 1% to 10% (Urey, 1952), so that in any case the spatial problem probably was never very serious. Moreover, there is some evidence that biological molecules may interact over distances many times greater than the interatomic distances which limit simple inorganic reactions (Rothen, 1956). Turing (1954) showed formally how differential aggregation, leading to morphological pattern, could arise spontaneously in simple metabolizing systems. Spatial differentiation inside cells would be an important aspect of evolution as soon as discrete organisms had formed.

There is not space here to consider subsequent evolution in detail, though it presents some formal problems as well as much fascinating detail. There would be automatically selective advantage in any increase in efficiency of coacervates as self-propagating systems, and in spirit this is already recent evolution. Increased stability and selectivity of the membrane, and increased size, are among the more important of such developments. As already noted, the latter may be spontaneous. The maximal cell-size is limited by physical forces (Rashevsky, 1940), and division is spontaneous at a critical size. Living organisms were able to circumvent this limitation of body-size by the evolution of the metazoon and metaphytan conditions. Turing (1954) envisaged that his postulated spontaneous morphogenetic heterogeneity would be valid also in a metazoon organization, and this organization permitted not only the eventual reperfection of all that had been achieved in the most efficient single cell, but also much novelty, mainly associated with increasing size. Its full exploitation, unlike that of the initial chemical ingredients (p. 196), has been a sustained process, since often a new advance has selective advantage only relative to previous advances in other stocks or species. In some cases it may have been a most probable event already at an early stage, but at that time its survival may not have been probable enough.

With each advance the phenomenon of death, a sudden discontinuity of function and a major disturbance of organization, became more spectacular. This phenomenon no doubt arose as soon as there was any kind of permanent fabric which could be effectively disorganized as a functional unit. It may have been a reality even before discrete coacervate bodies evolved (p. 197), but it became spectacular in proportion to the size of the system and to the degree of isolation of the organism from its environment—in proportion to the potential difference or "discontinuity" it had established relative to the environment. As a simple example, a large terrestrial animal which had acquired the ability to climb would risk death by falling in proportion both to its size (weight) and to the height to which it could climb. An increased potential difference automatically exposes the organism to new "hazards of discontinuity," and these may be increasingly of the all-or-none type—life or death matters. In a sense death is a most probable event, in this new situation of larger, even if possibly fewer, hazards.

To survive, the species must replace these lost individuals, just as the individual replaces molecules lost by wear and tear (p. 197). While in this sense individual death is an unavoidable misfortune, a certain death-rate is of positive selective advantage to the species. Without it there would be either serious stock-piling of individuals (p. 202) or a serious restriction of reproduction, and so of the genetic recombination so essential for small-scale, tactical evolution. The actual death-rate is a compromise between these alternative requirements; there is no selection against senescent changes after the effective reproductive age (Medawar, 1957).

Growth of the individual is necessary because reproductive replacement of individuals is necessary. Were it not for this an unchanging mor-

phological steady-state presumably would be the ideal, and it is perhaps correct to think of growth as a property of this adventitious kind rather than as a property necessarily more primitive than any other physiological function, as classical theory tended to suppose. Indeed, metabolism must certainly be a more ancient, and probably the only, pristine function. Today competition between organisms for materials (pp. 201–202) has become so intense that the replacement of losses by "maintenance" (p. 197) and by growth seems to be a main preoccupation, particularly of autotrophs; this is perhaps the main reason why the endergonic, rather than the exergonic, aspect of the organic cycle has seemed to be the limiting factor (p. 202).

If growth is not a peculiarly pristine property, then it is anything but a self-evident axiom that particular components of the living system must be capable of autosynthesis, though equally certainly the system as a whole must be self-propagating. Hinshelwood (1956) has pointed out, in the light of experimental evidence, that the individual components, proteins and nucleic acids, probably are not self-reproducing, but only the system as a whole. In the pristine system a mere statistical replacement, however indirect, would be adequate (p. 197). It does not necessarily follow that in modern organisms biosynthesis does not occur by a direct template type of copying of particular molecules, but only that such copying may require outside help, e.g., from nucleic acid in the case of protein synthesis. It is possible to visualize how the wear-and-tear type of metabolic exchange (p. 197) could also serve, under appropriate conditions, as a template mechanism for the neoformation of materials. The components *A*, *B*, *C*, . . . , instead of simply and individually replacing their identity in the fabric, might marshall themselves each alongside its identity, and then unite to form a completed copy.

Only the minimal requirements for the survival and evolution of a living system have been considered here, viz., that it must assimilate material and solar energy (autotrophs) or material already energized in this way (heterotrophs), so as to maintain a steady-state activity. This includes repair, growth, and reproduction where necessary. In addition many refinements of survival value have been progressively acquired, particularly by heterotrophs, and the explanation of these, as most probable processes, calls for future consideration. The most important acquisitions were overt movement and responsiveness, or "irritability." The former is seen in the movements of viruses and of mitochondria, in protoplasmic streaming and related activities, in cell division and in the familiar locomotor activities of pseudopodia, cilia, and muscles. The possibility that all depend on a common fundamental property is progressively strengthening as evidence accumulates (Weber, 1958). This property is the ability of protein molecules to use part of their energy not simply for chemical purposes but also for organized, massive physical processes, of which mass-movement is probably the simplest. The energy is regularly restored by such agents as adenosine triphosphate. Essentially similar molecular movements have now been postulated as a basis for secretion and other physiological activities, in addition to overt movements. These molecular movements may prove to be as spontaneous under appropriate natural conditions as the aggregation tendencies already noted (pp. 202–204), and including syngamy and ingestion. Even in living systems as highly evolved as the sarcodine Protozoa the processes of ingestion and locomotion remain very intimately related. In these animals the response function also remains very closely associated (Goldacre, 1952) and although probably in the interests of speed, small inorganic ions have now assumed the overt role in nerve conduction, this function also may be based on the same fundamental power of orientating and organizing the energy of protein molecules. There is no reason to think that this is not also a most probable property. The various "work-functions" have probably evolved from it spontaneously and without discontinuity.

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ON THE BROAD CLASSIFICATION OF ORGANISMS

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ABSTRACT

A system of broad classification which recognized a plant kingdom of four divisions and an animal kingdom of ten to fifteen phyla was for many years stable and standardized. Significant changes have occurred, or are now proposed. Among these, three major lines of development are discussed:

a. Classification of the algae has been fundamentally revised; seven or more algal series are distinguished primarily by characteristics of cells. The phylum concept, long established in zoological classification, has been brought into botanical classification in the systems of Pascher (1931) and Tippo (1942), in which the major algal series, the Bryophyta, and the Tracheophyta are regarded as phyla.

b. Many authors have advocated recognition of a kingdom of lower organisms, to meet the difficulty of dividing these between the plant and animal kingdoms. Two major possibilities for such a third kingdom are the Protista of Haeckel (1866, 1894), essentially identified with the unicellular organisms, and the Proctoctista of Hogg (1860) and Copeland (1947, 1956), comprising the nucleate, "acellular" organisms, including protozoa, algae, and fungi.

c. Study of the fungi has led to the view that these are probably derived from colorless flagellates as a line of evolution independent of true plants. The bacteria are better regarded as an ancient complex of many nutritive types, than as a group derived from the blue-green algae. In natural communities bacteria and fungi together form a major functional group (reducers) distinct from the green plants (producers) and animals (consumers).

It is consequently appropriate to conceive the broad relations of the living world in terms of three modes of nutrition and directions of evolution rather than two—the photosynthetic of the green plants, the ingestive of the animals, and the absorptive of the bacteria and fungi. These three directions of evolution appear on three major levels of organization—the Monera, or bacteria and blue-green algae, which lack nuclear membranes; the Eunucleata, or unicellular organisms with nuclear membranes; and the multicellular and multinucleate higher plants, animals, and fungi.

On this basis four kingdoms are here proposed: the Protista, or unicellular organisms; the Plantae, or multicellular plants; the Fungi; and the Animalia, or multicellular animals. Among the Protista the subkingdoms Monera and Eunucleata are distinguished. Among the higher organisms the less widely successful lines of evolution into the multicellular and multinucleate conditions are recognized as the subkingdoms Rhodophyta and Phaeophyta among the plants, Myxomycota among the fungi, and Parazoa and Mesozoa among the animals.

Other alternatives to the traditional two-kingdom system are discussed. Despite the general acceptance of the two-kingdom system, these alternatives have value in expressing current understanding of the broad relations among organisms. They should be judged in comparison with the two-kingdom system and with one another for their relative success in embodying these relations in a "natural" classification.

FOR many years a pattern of broad classification of the living world was stable and generally accepted. In this system, the living world consisted of two kingdoms, the plant and animal. In the plant kingdom, in a system for which Eichler (1886) is often given credit, four divisions (Thallophyta, Bryophyta, Pteridophyta, and Spermatophyta) were distinguished, with the Thallophyta divided into the classes Algae and Fungi. A somewhat larger number of phyla, generally between ten and fifteen, was accepted in the animal kingdom. This system, and evolutionary relationships thought to underlie it, are represented diagrammatically in Fig. 1. It was recognized that the division Thallophyta was a broad collection of forms of uncertain relation, that the plant divisions and animal phyla probably did not correspond as taxonomic units, and that the distinction of the kingdoms encountered difficulties among the microorganisms. Yet the system itself was so much a part of most biological thinking and teaching that departures from it often seemed radical and peripheral, the idiosyncrasies of individuals. Significant changes have now occurred, however, and others are proposed, affecting even the basic dichotomy of the living world into plant and animal kingdoms. Perhaps the time has come when alternative viewpoints can be considered on their merits, with as little concern as possible for their radicalism or heterodoxy. It is the purpose of this paper to discuss certain developments in the broad classification of organisms—the manner in which major relationships in the living world are expressed in taxonomic units on the highest levels—and to consider especially views on the kingdoms of organisms.

Three major lines of conceptual development affecting broad classification will be discussed, leaving aside developments affecting the animal phyla and their relationships which have been discussed elsewhere (Hyman, 1940; Hadži, 1953; Marcus, 1958). The first of these three, which is the most recent in origin but now the most widely accepted, concerns the revised classification of the algae and the plant kingdom. In many earlier classifications the algae were a class or subdivision of the division Thallophyta. Within the algae such major groups as blue-green, green, red, and brown algae and diatoms were distinguished. Revision of this view has resulted primarily from the detailed study of algal cells, especially such characteristics as the number, kind, length and arrangement of flagella, and the pigments, cell walls, and food storage forms. By these characters eleven or twelve series, or when some of these are grouped together (see Table 1) seven series, of algae may be recognized (Pascher, 1914, 1931; Smith, 1933, 1938, 1951; Fritsch, 1935, 1944; Papenfuss, 1955). Several of the series—Chrysophyta, Pyrrophyta, Euglenophyta, and Chlorophyta—have as their "central" members photosynthetic, flagellated unicells, by the characteristics of which the series may be characterized. Pigments and flagella cannot, however, be used to bound the series as wholes or describe all their members. In each series the photosynthetic flagellates are closely allied on the one hand wth non-motile, photosynthetic organisms which seem clearly to be true, though simple, plants; and these same flagellates are allied on the other hand with colorless, motile forms and, in the Chrysophyta and Pyrrophyta, with amoeboid forms feeding in part by ingestion—unicellular animals or protozoans. Each of the series includes varied evolutionary experiments in cell organization, nutrition, and

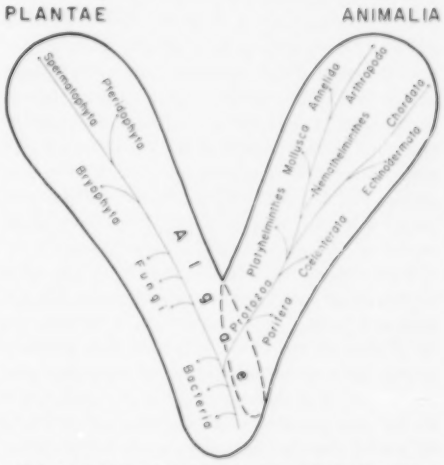

FIG. 1. A SCHEMATIC VIEW OF THE PLANT AND ANIMAL KINGDOMS AS THEY MIGHT HAVE BEEN CONCEIVED IN 1925, WITH EVOLUTIONARY RELATIONS OF MAJOR GROUPS SUGGESTED IN A SIMPLIFIED FORM

colony formation which are divergent from one another within the series, but convergent between series (Smith, 1933, p. 7; Fritsch, 1935, p. 26). Each series seems to represent an evolutionary line of great antiquity, within which there has been extensive diversification of form and function.

Smith (1933, 1950) has suggested that the major series represent six kingdoms of plant-like organisms, one of which, the Chlorophyta, has been far more successful in the evolution of higher forms than others and includes as divisions the green algae (Phycophyta or Chlorophyta s. s.), the Bryophyta, the Pteridophyta, and the Spermatophyta. Conard (1939) has treated the same groups as the seven divisions or phyla of the plant kingdom; Bessey (1907, 1914) had recognized a larger number of phyla. A pattern of classification which has been more widely adopted was proposed by Pascher (1931) and Tippo (1942). Pascher (1931) proposed eight algal series as *phyla*—the primarily unicellular Cyanophyta, Chrysophyta, Pyrrophyta, and Euglenophyta, the unicellular and multicellular Chlorophyta, and the multicellular Charophyta, Phaeophyta, and Rhodophyta. The division Bryophyta became a phylum of the same name, while the divisions Pteridophyta and Spermatophyta were tentatively united in the phylum Pterido-Anthophyta, the vascular plants. Tippo's (1942) proposal for a modern classification of the plant kingdom is in a similar pattern, following Smith (1938) in treatment of the algae, and Eames (1936) in that of the vascular plants, the phylum Tracheophyta. A suggestive parallel with the animal kingdom was observed (Fuller and Tippo, 1949) in connection with these proposals—that the Thallophyta might well be compared with the "invertebrates" as an assemblage of rather unrelated series of lower forms.

Two aspects of this revolution in botanical classification have most bearing on the present paper. The first is the application of the concept of *phylum* to the plant kingdom. The phyla of zoologists and the divisions of botanists had much the same function in broad classification of the two kingdoms, yet the concepts differed in a way that is expressed in the words themselves. The divisions were essentially that—groups into which the plant kingdom was *divided* by particular characteristics and without, in the case of the thallophytes at least, much concern for the evolutionary unity of the resulting divisions. The phyla are, in contrast, groups of organisms clustered together by similarity of organization and presumptive common evolutionary background. A phylum should be a *phylon* in the sense of a tribe of organisms related by common descent, as well as a taxonomic unit. The organisms of a phylum should share, or show derivation from, a common ground-plan or scheme of organization; and the phylum should be subject to definition by certain structures or characteristics shared by most or all of its members, and distinguishing them from members of other phyla. Any group of organisms, however small, which are so characterized may be regarded as a phylum until evidence is found demonstrating their relationship to another phylum (cf. Hyman, 1940, p. 32). Such an approach to definition may lead to an inconveniently large number of phyla, and to the appearance of some phyla of very small content—one or a few genera—such as are familiar in the minor worm phyla of zoologists and appear in some current botanical classifications (e. g., Bold, 1957). Divisions and phyla thus represent two major modes of classification—division of the objects or events being classified into classes by dichotomies based on particular characteristics, versus the search for clusters of forms related in their whole patterns of characteristics among the objects being classified. The latter, which may give results that seem less orderly, is more nearly the "natural" classification the biologist seeks. The difference of approach is no less significant for the fact that many botanists use the term "division" for the units of the newer classification.

The second consequence of the changed botanical classification is in the interpretation and taxonomic assignment of the flagellates. If the photosynthetic flagellates are central members of a number of phyla of plants or plant-like organisms, they are scarcely to be regarded as simply one-celled animals with choroplasts. More broadly, motility by flagella is not an animal character; it is essentially indifferent in relation to the plant and animal kingdoms. The flagellates are, rather than simply a class of protozoans, the central complex or matrix of the unicellular organisms with bounded nuclei. They are the true crossroads of the kingdoms, a group which presumably originated from un-

known forms allied to the bacteria and blue-green algae, and from which radiated not only the more distinctive unicells, but also the various lines of evolution leading to multicellular and multinucleate organization. Erection of four phyla which include pigmented flagellates results in a notable discordance between botanical and zoological classifications; for these same flagellates appear in certain orders within the class Mastigophora, phylum Protozoa, of zoologists. This discordance may perhaps best be resolved by separating the photosynthetic flagellates, and the colorless forms clearly allied with them, from the class or phylum of animal-like flagellates. The latter grouping, the Zoomastigina or Euflagellata, then includes various free-living, flagellated and flagellate-amoeboid forms feeding by absorption or ingestion or both—monads, choanoflagellates, and Rhizomastigina—as well as more complex, distinctive, and even bizarre forms which are mostly symbiotic with higher animals—trypanosomes, Polymastigina, and Hypermastigina.

Since the other phyla including flagellates contain both plant-like and animal-like members, the old difficulty of dividing the unicells between the kingdoms is not thus solved. The second line of conceptual development to be discussed is the attempt to solve this problem by erecting a third kingdom of lower organisms. The two-kingdom conception had its origin in the circumstance that man has always observed about him, on land, two clearly separated groups of organisms—the higher plants and higher animals. It was natural to abstract from these two groups generalized conceptions of plants as sessile or rooted, non-motile organisms, and of animals as organisms which mostly move about to obtain other organisms as food. Given the prepossession that the living world should thus be divided into two halves, biologists were able to assign macroscopic aquatic forms, fungi, and microorganisms to one or the other by characters they shared with higher plants or animals. Yet it was evident that the dichotomy originated to fit the higher organisms had to be imposed upon the microorganisms with results that were never very satisfactory, though long tolerated.

The alternative possibility of separating lower organisms into another kingdom was suggested by a number of early naturalists (see Bütschli, 1880–1882; Wilson and Cassin, 1864). Bory de Saint-Vincent (1825, 1828) proposed a *Regne Psychodiaire* for organisms (coelenterates and sponges) which are sessile and plant-like in one, and motile and animal-like in another part of their life-cycle. The idea of a third kingdom, as it influences current authors, originated in four separate proposals made in the brief period from 1860 to 1866. Owen (1860) observed that there are numerous organisms, mostly of minute size, which lack the distinctive characteristics of true plants and animals; for these (unicellular organisms in general, plus the sponges) the kingdom Protozoa (or Acrita, Owen, 1861) was proposed. Hogg (1860), observing the blending together of the plants and animals among the lower forms, proposed for these the *Regnum Primigenum* and the term "Protoctista." Wilson and Cassin (1864) proposed a third kingdom, Primalia, for the lower organisms. The best-known proposal, however, that of Haeckel (1866, 1878, 1894), was to separate lower organisms lacking tissues into the kingdom Protista. Although this kingdom at one time (1866) included the sponges and at another time (1878) the fungi, it comprised primarily, and in a later treatment (1894) only, the unicellular organisms. Haeckel's proposals were opposed by Bütschli (1880–1882) and others; and, although the term "protist" was widely used, the idea of a third kingdom lay dormant for the greater part of a century.

Suggestions along this line were made by Dobell (1911), E. B. Copeland (1927), and H. F. Copeland (1938, 1947). But in the last few years the idea has emerged from dormancy to appear in a number of books (de Laubenfels, 1949; Carter, 1951; Langeron and Vanbreuseghen, 1952; Deflandre, 1952; Moret, 1953; Copeland, 1956; Cameron, 1956; Stanier, Doudoroff, and Adelberg, 1957; Simpson, Pittendrigh, and Tiffany, 1957), including textbooks presenting the idea as a respectably established alternative to the two-kingdom system. The content assigned to this third kingdom varies, with two major alternatives. (1) The kingdom of lower organisms may be identified with the unicellular organisms, together with colonial forms derived from them, the kingdom Protista of Haeckel (1894; Deflandre, 1952; Moret, 1953; Simpson et al., 1957). (2) The kingdom of lower organisms may be defined to include other forms which are not unicellular but seem not to be

true animals or true, higher plants—the fungi and red and brown algae (Copeland, 1947, 1956), or these plus the green algae (Barkley, 1949; Rothmaler, 1948; Moore, 1954; Stanier et al., 1957). The term *Protista* has also been used in this second sense (Copeland, 1938; Moore, 1954); but it may be advantageous to follow Copeland's later use of Hogg's term *Protoctista* for this conception of a kingdom of lower, "acellular" organisms (Copeland, 1947, 1956). Authors accepting this conception generally separate the bacteria and blue-green algae from other lower organisms as the kingdom or subkingdom Monera or Mychota.

The third line of development concerns interpretation of the saprobic organisms and their relation to the plant kingdom. In a two-kingdom conception the fungi are grouped with the plants on account of their non-motile habit and development of walls around their protoplasm. Moreover, similarities between the structures and reproductive mechanisms of fungi and certain algae suggested that the fungi were a polyphyletic collection of forms derived from the algae, especially from syncytial filamentous forms (e.g., *Vaucheria*) and the Rhodophyta. Contributions to this viewpoint, and later changes in the interpretation of the fungi, have been reviewed by Martin (1955). The hypothesis that fungi are derived from algae has not been wholly abandoned (Bessey, 1942, 1950, 1955; Jackson, 1944); but as primitive fungi have become better known, the evolution of the fungi from colorless, flagellated ancestors has come to seem more probable (Atkinson, 1909; Cavers, 1915; Scherffel, 1925; Cook, 1928; Martin, 1932, 1936; Linder, 1940; Heim, 1952). A most likely pattern of evolution appears to be (a) independent derivation of several groups with a primitive fungal, or "chytrid" organization from protozoan ancestors, (b) derivation from two of these chytrid lines of the remaining Phycomycetes, (c) derivation of the Ascomycetes from one of these phycomycete lines, and (d) of Basidiomycetes from Ascomycetes (Smith, 1938, 1955; Moreau, 1954; Savile, 1955). The higher fungi thus appear to be an evolutionary line quite separate from the true plants.

This probability, together with the fact that organization of the fungi is fundamentally different from and non-homologous with that of the higher plants, has led some mycologists to conclude that fungi should not be regarded as plants (Martin, 1936, 1940, 1941; Langeron and Vanbreuseghen, 1952; Scott and Ingold, 1955, p. 122). Fries (1821) suggested a *Regnum Mycetoideum*; and a century later Conard (1939) proposed dividing the living world into the three kingdoms, Phytalia, Animalia, and Mycetalia. Dangeard (1899) observed that higher plants and fungi represented different evolutionary lines characterized respectively by photosynthetic and absorptive nutrition. Rogers (1948) has compared the evolutionary tendencies of the three groups—plants, animals, and fungi—and summarized their different directions in the solution of major problems: (1) water loss—cutin or cork, chitin or stratum corneum, escape with hidden hyphae; (2) asexual reproduction—zoospores and tetraspores, absent in most forms, zoospores and conidia; (3) nutrition—photosynthesis, cytophagy, diffusion only; (4) generations—extension of diploid and reduction of haploid, early loss of haploid, reduction of haploid and interpolation of an extensive dikaryotic phase; (5) fertilization—microspore motility, copulation, loss of gamete motility associated with gametangiogamy and somatogamy. Rogers (1948) concluded that the contrasts are evidence for the propriety of maintaining the three groups as mutually distinct.

A related viewpoint has developed among ecologists. In the natural community three major groups of organisms may be recognized. The producers are autotrophic organisms which take inorganic substances from the environment and, using light or chemical energy, synthesize all the organic compounds of their protoplasm. The consumers are those heterotrophic organisms which consume, or ingest, the living or dead organic matter of other organisms; in community function they harvest, directly or indirectly, part of the productivity of the producers. The reducers are those other heterotrophic organisms which also utilize organic matter, but do so by absorption. By decomposing the dead remains of other organisms, the reducers release back into the environment inorganic products of this decomposition, some of which are then available for uptake by the producers and recirculation through the community.

The three groups—producers, consumers, and reducers—correspond in general to three major modes of nutrition—holophytic, holozoic, and

saprobic—and three or four major groups of organisms. In most present communities the producers may be largely identified with the photosynthetic green plants. The consumers, feeding by ingestion and digesting their food within themselves, are the animals. The reducers may be largely identified with bacteria and fungi. Free-living absorption has been closely linked with absorptive parasitism in the evolution of these reducers, however; and absorptive organisms include many parasitic bacteria and fungi, as well as many forms evolved from animals and green plants. One may, however, regard the producers, consumers, and reducers as following three major modes of life and ways of obtaining energy, as three "functional kingdoms" in natural communities (Whittaker, 1957; Odum and Odum, 1959).

As such, these modes of nutrition are also three major directions of evolution which are expressed in the characteristics of organisms. These characteristics may be recognized among both unicellular and multicellular or multinucleate organisms, but are most clearly differentiated among the latter, especially in their evolution on land. Thus, with the photosynthetic nutrition of plants is correlated a stationary and inactive life, with much or all of the plant body exposed to light. In relation to this way of life, the cells possess chloroplasts and rigid cellulose walls; and in the higher forms, there is differentiation of the plant body into (a) rhizoids, holdfasts, or roots for attachment and (on land) for obtaining water and nutrients, (b) stipes or stems for support, (c) expanded blades or leaves, and (d) vascular tissues to interrelate these. The life of animals is characterized by motility, related to the seeking and ingestion of food. Correlated with food-seeking and ingestion are mostly wall-less cells, often combined with a distinct skeletal system, and an evolution of characteristic organelles and organs for sensory perception, nervous transmission, and movement, food-processing and circulation. Saprobes characteristically live inactively embedded in the medium or food source, into which enzymes may be excreted and from which food is absorbed. Correlated with this mode of existence are two relatively simple types of organization—the single cells of the bacteria and yeasts, and the syncytial mycelia of the higher fungi—contrasted, in the higher forms, with the evolutionary elaboration of reproductive structures and life cycles.

The fact that saprobes have evolved within the limits of these two relatively simple types of organization (and the less widely used chytrid and plasmodial types) has been one reason for their inclusion among the plants. They appeared to form two relatively homogeneous groups, which are specialized for saprobic nutrition, among the larger number of groups of thallophytes. Yet this simple structure is itself a correlate of the saprobes' way of life. Since saprobes live within the food source and feed by absorption through the whole surface, there has been little evolutionary reason for the differentiation of organelles within the cells of bacteria, or tissues and organs within the mycelia of higher fungi. Yet the unity of the bacteria and fungi as taxonomic groups may be superficial. The chemical specialization among the bacteria, and the chemical and reproductive diversity among the fungi, suggest that these organisms have been subject to evolutionary diversification—in those characteristics in which there was most reason for diversification—of significance comparable to that seen in the plant and animal kingdoms. The bacteria are often referred to as one-celled fungi, but a saprobe kingdom comprising the bacteria and other saprobic unicells and the fungi may not be a "natural" group in the sense of evolutionary continuity. The mycelial bacteria are generally thought to be only convergent with the mycelial fungi. Yet this convergence of organization (along with such interesting, if more incidental reproductive convergences as the conidia and "zoosporangia" of mycelial bacteria, a type of budding in certain bacteria, and the slime-mold-like reproduction of the slime bacteria) may support the justice of grouping the bacteria and fungi together as representing a third major way of life and direction of evolution.

Changing views on the origin and early evolution of life bear also on the place of the absorptive organisms in the living world. It was once thought that, since most organisms are now dependent on green plants for their food, all saprobes must have evolved later, from green plants. The heterotroph hypothesis on the origin of life developed by Oparin (1938) and others paints a different picture. According to

this view, simple organic compounds, produced by the action of light and electricity on water, methane, ammonia, and other substances, are thought to have accumulated in the ancient seas. The earliest "organisms" were heterotrophs which used these substances in the construction of increasingly complex, self-reproducing chemical systems which eventually became protoplasm organized in cells. The earliest cellular organisms probably fed by absorption and depended upon the energy available from absorbed organic compounds. Chemoautotrophic nutrition, using reactions with absorbed inorganic substances to obtain the energy of life as the supply of organic compounds in the sea water declined, was probably a next major development. Photosynthetic nutrition was probably, though not certainly, a later accomplishment (van Niel, 1956) which eventually, when established in its modern form in organisms ancestral to the photosynthetic flagellates, became the evolutionary direction of the plants as we know them today.

It is unlikely that the bacteria as a group, excluding colorless counterparts of blue-green algae, are derived from these algae (Pringsheim, 1949). Some autotrophic forms capable of using carbon dioxide and an inorganic reducing substance for the synthesis of food in the absence of oxygen may be of very great antiquity, survivors from the oxygenless atmosphere of early evolutionary history (Urey, 1952). Many bacteria may be derived from such bacterial autotrophs, not from green plants. Yet, though the bacteria probably include many such secondary heterotrophs, it may not be true that all heterotrophic bacteria are secondarily so. An hypothesis of early evolution which assumes as stages —(1) primary heterotrophs only, (2) extinction of these, evolution of organisms photosynthetic in the modern, oxygen-releasing manner, (3) evolution of secondary heterotrophs—may be somewhat too simple. It is likely that natural communities of different environments in the remote past included forms of varied nutritive relations, among them some heterotrophs and chemoautotrophs; and it seems likely that some heterotrophic bacteria evolved along with the autotrophic forms, rather than being simply derived from them. Without assuming any existing bacteria to be survivors of the original, primary heterotrophs, one may consider that the bacteria represent an ancient complex, within which there occurred the first great radiation into different modes of life, among which heterotrophic forms may always have been present, and from which we now have some survivors of the early radiation, together with other forms whose means of nutrition are of more recent evolution.

Despite the obscurities of bacterial evolution, it may be suggested that the absorptive mode of nutrition was first evolved, that it may always have been important, and that the major groups of absorptive forms, the bacteria and fungi, are not simply secondarily absorptive plants. It is suggested also that this interpretation of the bacteria, the views of mycologists on the evolution and distinctiveness of the higher fungi, and the views of ecologists on the place of the reducers in natural communities converge toward a conclusion: the broad relationships of organisms in the living world are better conceived in terms of three modes of nutrition, ways of life, and directions of evolution, than two.

This conception has been expressed in a diagram, Fig. 2, representing the three directions of evolution in relation to the three levels of diversification. The open space below the Schizomycetes represents the unknown, precellular ancestors of the bacteria; to this might be assigned the earliest heterotrophs and, possibly, the viruses. The first level of diversification among cellular organisms is that of the "Monera" or lower protists, one-celled organisms lacking nuclear membranes. Among them saprobic bacteria predominate; but diversification on this level includes also the chemosynthetic bacteria, the photosynthetic bacteria and blue-green algae evolved in the direction of the plants, and other types of organization—cellular filaments or trichomes and hyphae—convergent with the other algae and the fungi. There are no "animals" on this level; but the spirochaetes are animal-like in motility, and some of the slime bacteria appear to be absorptive "predators" on other unicellular organisms. The second level of diversification is that of the nucleate, one-celled organisms, the "Eunucleata" or higher protists. The most characteristic type on this level is the free-swimming, flagellated cell. These flagellates include forms which are holophytic, saprobic, or holozoic, or vari-

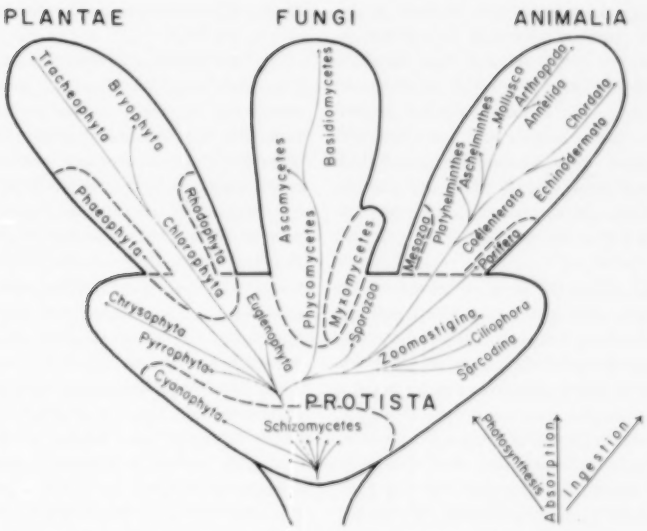

FIG. 2. A SCHEMATIC VIEW OF THE KINGDOMS AND PHYLA ACCORDING TO THE VIEW PRESENTED HERE, WITH EVOLUTIONARY RELATIONS SUGGESTED IN A SIMPLIFIED FORM

Many of the animal phyla are omitted. No derivation is indicated for the Rhodophyta, Myxomycetes, and Mesozoa, and none is implied by the positions of the Euglenophyta and Sporozoa near these groups.

ously myxotrophic in nutrition; and the diversification on this level includes also algae and protozoa which have further diverged in the plant and animal directions of evolution, as well as various experiments in the formation of colonies and syncytia other than those which led to higher organisms. The third level of diversification is that of the multicellular and multinucleate higher plants, fungi, and animals, among which the three directions of evolution are most clearly distinguished, even though there are partial, secondary convergences—absorptive higher plants and animals and "carnivorous" plants and fungi.

By the argument of this paper, Fig. 2 offers a better representation of broad relations among organisms than Fig. 1. Even so, there are various possibilities for the classification of living things into kingdoms. Four major lines of division are suggested by this scheme—between the Monera and the Eunucleata, between the Eunucleata and the multicellular and multinucleate series, between plants and fungi, and between fungi and animals. These and the additional possibility of the "Protoctista" admit of a number of ways of bounding and defining kingdoms. Some of the possibilities are as follows:

(1) The unity at the biochemical level of the living world, and the difficulties affecting each of the ways of dividing it, may lead to a view that there is really only one kingdom. Thus Walton (1930) has proposed a kingdom Bionta, divided into three subkingdoms—the Protistodeae or protists, the Metaphytodeae or true, multicellular plants, and the Metazoodeae or multicellular animals. Dillon (1957) has recently presented a system based on a study of internal cellular structures throughout the living world, and a resulting scheme of relationships different from Fig. 2. Dillon considers that there is, in effect, but one kingdom of living things, the plants, forming an evolutionary tree from which the lower animals and saprobes have branched off at various points, and of which the higher animals and fungi are also branches. Walton's system makes clear one limitation of a one-kingdom system—the problem of division, since division is necessary to classification, is simply transferred down a level, to the subkingdoms. And, although the statement that the fungi and animals are branches from a broadly defined plant kingdom is surely a reasonable one, yet one may still consider that

one or both of these branches is of such importance as to merit recognition as a kingdom.

(2) One may draw a vertical line between fungi and animals and downward through the protists on Fig. 2. The result is the traditional two-kingdom system comprised, on the one hand, of animals, most generally characterized by motility and ingestion of food, and on the other, of plants, characterized by non-motile life and absorption of inorganic or organic substances or both.

(3) A second oblique line may be drawn between the plants and fungi and down through the Protista between the blue-green algae and bacteria. Three kingdoms characterized by nutrition and correlated features would result—multicellular plants plus unicellular algae; fungi plus bacteria and other saprobic unicells; and multicellular animals plus protozoa. This system, which is essentially that of the ecologists' functional kingdoms (Conard, 1939; Whittaker, 1957; Odum and Odum, 1959) has much to recommend it. It also, however, compounds the difficulties of classification among the unicellular organisms.

(4) The true, green plants may be assigned to one kingdom characterized by green color without masking pigments, and the true, multicellular animals to another kingdom characterized by gastrulation and a digestive tract. Other organisms possessing nuclear membranes become the kingdom Protoctista, those lacking nuclear membranes the kingdom Monera. This, with variations in detail, is the system adopted by Copeland (1938, 1947, 1956), Rothmaler (1948), Barkley (1949), Moore (1954), Cameron (1956), and Stanier et al. (1957). The kingdom Protoctista is of recognized heterogeneity (Copeland, 1938), and is subject to criticism as a collection of organisms excluded from the other kingdoms by the definitions chosen for them, a group lacking coherent meaning in its own right (Whittaker, 1957). A definition of the plant kingdom which excludes the higher algae on the basis of pigments other than chlorophyll may seem an arbitrary one; and the conception of a "protist" or lower organism may be somewhat stretched when it is applied to brown algae and basidiomycetes. Yet this system avoids better than any other the problem of transitional groups which may belong to more than one kingdom, and leads to kingdoms which are "natural" in the sense of probable monophyletic origin, at least.

(5) The unicellular organisms may be grouped into the kingdom Protista, while the higher organisms, in which major directions of evolution are expressed in multicellular or multinucleate structure, become the kingdoms Plantae, Fungi, and Animalia. This four-kingdom conception is the author's preference, as expressed in the terms in full capitals in Fig. 2. Possible variations on it include grouping of the fungi with the plants (Simpson et al., 1957), separation of the lower and higher protists as kingdoms, and recognition of a kingdom for the non-cellular viruses.

(6) The kingdoms might be based on levels of organization: the non-cellular (Archebionta or Archetista), the Monera, the Eunucleata, and the higher organisms, which might be collectively termed the Metabionta (= Histonia, Haeckel, 1904). Thus Haeckel (1904) in a later work proposed the kingdoms Protista and Histonia, each including subkingdoms of animal-like and plant-like organisms.

(7) A number of kingdoms larger than four may be recognized. E. B. Copeland (1927) suggested there are a number of minor kingdoms in addition to the three major ones—plants, animals, and bacteria. Jahn and Jahn (1949) have proposed a six-kingdom system comprised of the Archetista (viruses), Monera, Protista (protozoa and nucleate algae), Fungi, Metaphyta, and Metazoa.

One of these possibilities, the four-kingdom system described under (5), was suggested by the author (1957), but is here formally proposed for the first time. Table 1 outlines a classification of the living world down to phyla by this system. Some familiar synonyms are given, but no effort has been made to give full synonymies and authorities, which are summarized for the Protoctista by Moore (1954) and Copeland (1956), and in other treatises for plants and animals. Some possibilities of broader versus narrower definition of phyla are suggested by indenting certain groups which are treated as phyla by some authors, subphyla or classes by others. Where the indented taxa are in parentheses, they are groups which sometimes are, and sometimes are not, separated as phyla distinct from the remainder of the phylum under which they are listed. When the indented taxa are not in parentheses, they represent the

several phyla into which some authors divide the group under which they appear.

Two levels of organization—the bacteria and blue-green algae, and the higher forms with nuclear membranes—are distinguished among the Protista. Various authors (Copeland, 1938, 1947, 1956; Stanier and van Niel, 1941, Jahn and Jahn, 1949; Scott and Ingold, 1955; Breed, Murray, and Smith, 1957) have suggested separation of the bacteria and blue-greens into a kingdom Monera or Mychota. Recent studies of the nuclei or central bodies and genetic recombination in bacteria have reduced the sharpness of the contrast; yet the evolutionary connection of the two levels is unknown, and they represent two clearly separated levels of evolutionary diversification. Without disagreeing with the view of the Monera as a kingdom, the author has chosen the more conservative course of treating these and the protists with nuclear membranes as two subkingdoms. The term *Eunucleata* is here proposed for the latter. Under the Eunucleata have been listed three phyla of photosynthetic flagellates and their allies, and four groups of absorptive and ingestive protists treated as protozoan classes in many zoological classifications. Classification of the bacteria offers special difficulties (Kluyver and van Niel, 1936; van Niel, 1946, 1955); and classifications vary widely, especially in a wider versus narrower content of the Eubacteria (Pringsheim, 1923; Stanier and van Niel, 1941; Breed et al., 1948; Breed, Murray and Smith, 1957). The simple organization of the bacteria makes difficult the application of the phylum concept to them; yet it may be reasonable to consider that the bacteria include several phyla of rank coordinate to the Cyanophyta. Three major groups are indented as possible phyla in Table 1 (cf. Stanier and van Niel, 1941; Stanier et al., 1957), without attempting to assign places to the viruses, rickettsias, pleuropneumonia-like organisms, and distinctive groups of bacteria as in the classification of Breed et al. (1957).

The true fungi have been divided into the conventional three phyla. A different system based on chytrid lines and higher fungi derived from them has been proposed by Moreau (1954). The slime molds, with their amoeboid and plasmodial organization and nutrition by ingestion as well as absorption, represent a different line of evolution; the subkingdoms Eumycota and Myxomycota are consequently proposed here.

Among the plants there are two important lines of evolution into the multicellular condition independent of the true green plants. For these three evolutionary lines the subkingdoms Euchlorophyta, Rhodophyta, and Phaeophyta are proposed. The subkingdoms Myxomycota, Rhodophyta, and Phaeophyta and the animal subkingdoms Mesozoa and Parazoa thus have a common meaning in the scheme. They represent lines of evolution, with more or less distinctive types of organization, into the multicellular-multinucleate level which are less widely successful than the main lines of evolution of the Euchlorophyta, Eumycota, and Eumetazoa.

The algae and higher plants are otherwise classified in the pattern of Pascher (1931), Smith (1938), and Tippo (1942). Fritsch (1935, 1944) and others prefer to recognize some of the indented algal groups, especially the chloromonads and cryptomonads, as independent series or phyla. Other authors subdivide the Tracheophyta into four or more phyla; Bold (1957) recognizes the Ginkgophyta and Gnetophyta in addition to the seven groups listed. It may be argued that the condition of the "vascular plant" is no more a necessary indication of phyletic unity than such other widely successful designs as the mycelium, flagellated unicell, and unsegmented worm. Similarities of organization among tracheophytes, evolutionary linkages converging back to the psilophytes, and tracheophyte evolutionary history on land may, however, give point to an analogy of the phylum Tracheophyta with the two major groups of animals on land, the vertebrates and arthropods. It may be true both that the Tracheophyta can be regarded as a phylum no more inherently diverse than these great animal phyla, and that this diversity and importance in the plant kingdom will lead many botanists to prefer division of the tracheophytes into a number of phyla.

There is now fair agreement on phyla of the animal kingdom, apart from differences among authors on ranking of some of the indented taxa. Few zoologists now regard the lophophore-bearing animals as a natural phylum, but Hyman (1940) and others recognize the phylum Aschelminthes in current treatments. The tendency, however, is to regard the Tentaculata, Aschelminthes, or Nemathelminthes (Nematoda,

TABLE 1

A Suggested Classification of the Phyla of the Living World

Kingdom Protista
- Subkingdom Monera or Mychota
  - Schizomycota or Schizomycetes, bacteria
    - Eubacteria, unicellular, filamentous, and mycelial true bacteria
    - Myxobacteria, slime bacteria
    - Spirochaetae, spirochaetes
  - Cyanophyta or Myxophyta, blue-green algae and colorless derivatives
- Subkingdom Eunucleata
  - Chrysophyta
    - Xanthophyceae or Heterokontae, yellow-green algae
    - Chrysophyceae, golden-brown algae
    - Bacillariae or Diatomacea, diatoms
    - Chloromonadina, chloromonads
  - Pyrrophyta
    - Dinophyceae or Peridinieae, dinoflagellates
    - Cryptophyceae or Cryptomonadina, cryptomonads
  - Euglenophyta or Euglenoidina, euglenoid organisms
  - Sporozoa, sporozoans
    - (Amoebosporidia, Cnidosporidia, or Neosporidia)
  - Zoomastigina or Euflagellata, animal flagellates
  - Sarcodina or Rhizopoda, rhizopods
  - Ciliophora, ciliates and suctorians

Kingdom Fungi
- Subkingdom Myxomycota
  - Myxomycetes or Mycetozoa, slime molds
- Subkingdom Eumycota
  - Phycomycetes, alga-like fungi
    - (Archimycetes, chytrids)
  - Ascomycetes, sac fungi
  - Basidiomycetes, club fungi

Kingdom Plantae
- Subkingdom Rhodophyta
  - Rhodophyta, red algae
- Subkingdom Phaeophyta
  - Phaeophyta, brown algae
- Subkingdom Euchlorophyta or Chlorophyta
  - Chlorophyta or Phycophyta, grass-green algae
    - (Charophyta, stoneworts)
  - Bryophyta, mosses and liverworts
    - (Hepatophyta, liverworts)
  - Tracheophyta, vascular plants
    - Psilopsida or Psilophyta, whisk ferns and psilophytes
    - Sphenopsida, Arthrophyta, or Calamophyta, horse-tails and calamites
    - Lycopsida, Microphyllophyta, or Lepidophyta, clubmosses and lepidodendra
    - Pteropsida, Pterophyta, or Filicophyta, ferns
    - Cycadophyta, cycads, cycadeoids, and seed-ferns
    - Coniferophyta or Strobilophyta, conifers and cordaites
    - Anthophyta, flowering plants

Kingdom Animalia
- Subkingdom Parazoa
  - Porifera, sponges
- Subkingdom Mesozoa or Agnotozoa
  - Mesozoa, mesozoans
- Subkingdom Eumetazoa
  - Coelenterata, radiate animals
    - Cnidaria or Coelenterata, coelenterates
    - Ctenophora, comb jellies

TABLE 1—*Continued*

Platyhelminthes, flatworms
Nemertea or Rhynchocoela, ribbon worms
Acanthocephala, spiny-headed worms
Aschelminthes, pseudocoelomate worms
  Rotifera, rotifers or wheel animalcules
  Gastrotricha, gastrotrichs
  Kinorhyncha or Echinodera, echinodere worms
  Priapulida, priapulid worms
  Nematoda, roundworms
  Nematomorpha or Gordiacea, horsehair worms
Tentaculata or Molluscoidea, lophophore-bearing animals
  Entoprocta, pseudocoelomate polyzoans
  Bryozoa, coelomate, ectoproct polyzoans or moss animalcules
  Brachiopoda, lamp shells
  Phoronida, phoronid worms
Mollusca, molluscs
Annelida, segmented or annelid worms
  (Sipunculoidea, sipunculid worms)
  (Echiuroidea, echiurid worms)
Arthropoda, arthropods
  (Tardigrada, water bears)
  (Onychophora, *Peripatus*)
Pogonophora, pogonophoran worms
Chaetognatha, arrow worms
Echinodermata, echinoderms
Chordata, chordates
  (Hemichordata or Branchiotremata, tongue or acorn worms)

Nematomorpha, Acanthocephala, and Echinodera), and Coelenterata in the broad sense as groups of phyla of uncertain relationship, rather than phyla. Rank given to the groups in parentheses is more variable, but the unsegmented worm groups given under the Annelida are more generally regarded as small phyla. There are consequently twenty to twenty-eight well-established phyla of multicellular animals, a much larger number than in the multicellular plants and fungi. Phyla are not necessarily comparable units from one kingdom to another; but the difference may reflect a real contrast in the extent to which, in correlation with their different ways of life and types of organization, the organisms of the different kingdoms have diversified into major groups without clear relation to one another.

In conclusion, some points regarding this system in relation to problems of broad classification in general may be offered. First, it may be observed that none of the systems mentioned here is wholly satisfactory; each has advantages over the conventional system, but also its own inherent limitations. The living world is a realm of interrelations of both immense complexity and frequent obscurity or ambiguity, of numerous evolutionary lines diverging and converging, of organic characteristics only partially correlated and often independently attained, of continuities and discontinuities which are sometimes aids and sometimes obstacles to the recognition and delimitation of taxonomic units. It should be clear that the kingdoms of organisms are not simply inherent in this complexity. The kingdoms represent views of the very broadest and most widely significant relations, as they might be seen viewing the living world at a distance from which, while the complexities are not forgotten, they become subordinated to the large-scale pattern. The systems of kingdoms are products of human contemplation of the living world, and the collection of proposals as to kingdoms given above may have special point

in illustrating the diverse conclusions to which thoughtful biologists may be led in contemplation of the same living world. The various systems may be judged by their relative success in expressing those broad relationships which seem most important, and in achieving that which biologists imply by a "natural" classification.

Natural classifications have several, interrelated characteristics which may perhaps be formulated: (1) Taxa should be internally coherent, and subject to clear definition and delimitation. They should be based on consideration of a maximum number of characteristics, though a few "key" characters may be chosen for practical definition. So far as possible natural continuities or intergradations should be taken advantage of to unify, and natural discontinuities to separate, taxa. (2) Evolutionary unity, of common descent, should underlie the coherence of the taxa. Polyphyletic derivation of the organisms in a taxon is avoided when it can be recognized; but it is not necessarily excluded, especially in cases of parallel rather than convergent evolution. (3) The design of the classification as a whole should embody our understanding of major relations among organisms; it should organize the information about these organisms and summarize this knowledge in the way best comprehended. (4) In the number, arrangement, and ranking of taxa, the design should express (or should be generally consistent with) our current understanding of evolutionary relations.

The alternative criteria of naturalness—present similarity in a maximum number of characters, and common evolutionary descent—will not always simply coincide; and it is the former which is the primary objective for classification itself (cf. Turrill, 1942; Bigelow, 1956, 1958). Certain compromises with the ideals of natural classification must often be struck, as can be illustrated in the systems of Copeland and the author. The Copeland system is led by the quest for monophyletic derivation and sharp definition of the plant and animal kingdoms to an exclusion of the higher algae from the former and to production of a kingdom Protoctista which, though presumably monophyletic, lacks coherence. The author's system compromises the principle of monophyletic derivation in its kingdoms of higher organisms. The higher organisms are conceived of not as a tree of two trunks, but as a shrub of three major and several minor stems ascending from the protists. The minor stems are grouped with major ones by their direction of evolution, and are designated as subkingdoms. While the resulting kingdoms are polyphyletic products of parallel evolution, this design of the classification as a whole has been preferred to the alternatives—the creation of minor kingdoms and the merger of widely divergent minor stems into the Protoctista.

Most of the proposed alternatives to the two-kingdom system have in common the recognition of at least one kingdom of lower organisms. The concepts of "lower" and "higher" organisms are subject to well-known ambiguities (Franz, 1911; Dobell, 1911); and various problems of definition, and of continuity and discontinuity, arise in this separation. In the Copeland and related systems, the phylum Chlorophyta has been variously assigned to the Plantae and Protoctista; for the relationships are such that the Chlorophyta clearly belong both with the other algae on the one hand, and with the higher plants on the other. The most serious practical limitation on the author's system is the straddling by various phyla of the line between the unicellular and multicellular –multinucleate conditions. This line is clearest among the animals, though multinucleate forms occur in all protozoan phyla, and the sponges may be connected with the choanoflagellate protozoa. The phylum Chlorophyta effectively crosses the line, however; and all four phyla of fungi include unicellular forms (though the ascus- and basidium-producing yeasts may be thought secondarily unicellular). The Eunucleata include a large number of "experiments" in cell aggregation and simple syncytia; and there may be little logic in placing some of these (e.g., the Sporozoa and Foraminifera) on one side of the line, and others (chytrids and slime molds) which seem more closely related to higher organisms on the other. But the logic of the classification offered here is not affected by shifting one or more phyla of slime molds and chytrids to a position in the Protista adjacent to the Sporozoa and Zoomastigina in Table 1. Both the Archimycetes and Myxomycetes, as broadly defined, are probably polyphyletic groups united by type of organization, not common ancestry—as is probably true also of the four protozoan phyla.

There is, in the broad picture, no real discontinuity between the unicellular and the multicellular and multinucleate conditions which can be taken advantage of in bounding the kingdoms. The familiar difficulties of separating plant and animal protists are thus replaced by the different difficulty of separating protists from higher organisms. Nevertheless, the line separating organisms evolving and differentiating their protoplasm within the limits of a single cell, from those evolving, enlarging, and differentiating with multicellular or multinucleate organization is surely one of the most significant that can be drawn through the living world. In the system of Copeland the Protoctista are interpreted as not unicellular but "acellular" in the conception advocated by Dobell (1911) and supported by Hyman (1940) and Hutner and Provasoli (1951). A major point of Dobell's essay—that in the protists functional differentiation of protoplasm occurs without division of the protoplasm into cells, while in the higher organisms functional differentiation is based on cell differentiation—is an essential point of the line drawn between the unicellular and the multicellular and multinucleate in this paper. The point that the protists are consequently non-cellular organisms is a much more doubtful interpretive statement, as observed by Boyden (1957). And if the "unicells" are acellular in this sense, the fungi are acellular in a different sense, and the higher algae in still another sense—if, indeed, the brown algae can be said to be acellular in any sense at all. The identification of acellular organisms and Protoctista with tissueless organisms is questionable, considering the brown algae and the differentiation of somatic mycelial and reproductive tissues, at least, in the higher fungi. These observations may imply only that the distinction between acellular and cellular forms is subject to difficulties, as is that between unicellular and multicellular-multinucleate forms. But it is this author's view that an "acellular" conception less effectively clarifies the relations of higher and lower organisms, and the two major levels of organization on which evolution has proceeded, than identification of protists with unicellular organisms.

From this treatment of the protists, and the division of the higher organisms, there result four kingdoms which may be characterized and given coherence by their major evolutionary themes. The themes of the kingdoms Plantae, Fungi, and Animalia are the three major modes of nutrition, and the morphological and physiological traits correlated with these. The theme of the kingdom Protista is the unicellular condition itself—the cell as an organism, and the exploration of diverse chemical and structural possibilities within the limits of the cell. These themes are inconsistent in that three are nutritional modes and their expression in organization, while the fourth is a level of organization. Yet this arrangement is consistent with another principle of classification, in dealing with an ancestral complex and a number of evolutionary lines divergent from it. It is in general better to treat the ancestral complex as a taxon coordinate to those of the descendent lines, rather than to attempt to extrapolate the distinctions between descendent lines through the ancestral complex and assign its intergrading members to the taxa of descendents (Simpson, 1945). It is the author's view that the four-kingdom system developed here better satisfies this principle, the objectives of natural classification, and the expression in kingdoms and subkingdoms of the broadest evolutionary and ecological relations of the living world, than the two-kingdom system or other alternatives.

One may finally ask why, when the two-kingdom system is so firmly established, the various alternatives should be proposed. Relative stability, too, is an objective of classification, and no feature of classification has been so stable as this. Yet no part of science is immune to reconsideration, and it may well be that the strength of the two-kingdom system is more in tradition than inherent merit. Whether or not the alternatives remain minority viewpoints, there is value in considering these alternatives for the manner in which they embody, and thus clarify, current understanding of broad relations in the living world. If alternative systems may better serve this conceptual function, they may well be considered also for application in practice to the broad classification of organisms.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will occasionallly appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to H. B. Glass, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *Department of Biology, The Johns Hopkins University, Baltimore 18, Maryland, U. S. A.*

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GENERAL BIOLOGY: PHILOSOPHY AND EDUCATION

PRINCIPLES OF RESEARCH IN BIOLOGY AND MEDICINE. *By Dwight J. Ingle. J. B. Lippincott Company, Philadelphia and Montreal.* $4.75. xv + 123 pp. 1958.

This book is directed to "students who are preparing for or are beginning research in macrobiology and medicine." It would certainly serve this purpose well, but it is of even more significance to the seasoned investigator. It is essentially an insight into the personal convictions of an inspired and thoughtful scientist. His enthusiasm is real, constantly pervading, and very contagious. Ingle feels, for example, that research should be fun. "It should be possible for the scientist to find enjoyment in the laboratory and in the fellowship of scholars throughout his life." For the student, "science should be presented in all its color, beauty, and drama, indeed, the exciting qualities of science should not be lost from graduate education or from life in the laboratory, although discipline and attention to detail are also required."

Although "it is no more possible to teach a scientist how to make a great discovery than it is possible to teach a poet how to write a great poem, an artist to paint a great picture, or a composer to write a great symphony," it is possible to teach the general principles concerning the gathering of data and their interpretation. In this book, Ingle is concerned with these broader aspects of research which he finds personally entertaining and commonly neglected in scientific education. He discusses the general aims and basic logic of science, the limitations of the mind, causality, statistical design and analysis, and the relationship among scientists. But he also manages to caution against the multitude of minor sins and errors as well. He points out the inherent subtleties in the use of the common syringe, the random selection of animals, and even the proper care of technicians.

Throughout, he has delightfully enlisted the aid of Lewis Carroll and the immortal Alice to illustrate the frailties of the scientist, the rigors of the laboratory, and the limitations placed upon ideas. For the uninitiated, here is an unusual chance to observe the challenging and fascinating thinking of a great experimentalist; and for the established investigator, pause for some fresh and stimulating ideas.

SHERWOOD M. REICHARD

THE LOGIC OF SCIENTIFIC DISCOVERY.

By Karl R. Popper. Basic Books, New York. $7.50. 480 pp.; ill. 1959.

Popper has a loftier conception of the limitations of philosophy than most other modern philosophers. He believes philosophy should provide the rationale and touchstone for science. In this respect he is a throwback to the classical philosophers. He avoids the Cartesian statement with its implication of solipsism and thereby is able to ask the question of fundamental importance to modern science: what constitutes the proof of a theory?

The principle of causality is excluded from the sphere of science by Popper. He puts in its place an exhortation that we continue to search for a logical and casual scheme for the universe. He allows it to be said of a scientific theory only that it has not been proven false—yet. There is no ultimate truth that we can obtain, only theories that are less likely to be false.

The book is a translation by the author of the 1934 German edition, with extensive additions to bring it up to date. The style is quite smooth and readable. As an added feature, the book has a letter to Popper from Albert Einstein which is reproduced in Einstein's own handwriting.

One should read this book if only to formulate clearly his objections to it.

MYRON L. WOLBARSHT

THE PHYSICAL FOUNDATION OF BIOLOGY. *An Analytical Study.*

By Walter M. Elsasser. Pergamon Press, New York, London, Paris, and Los Angeles. $4.75. x + 219 pp. 1958.

This is a very disturbing book. It deals, as the title suggests, with problems of concern to every biologist. The author asserts no less than the existence of biological laws which are not deducible from the laws of physics. Such claims have been advanced before, but the present one is based on recent developments in physics (theory of measurement on objects of atomic dimensions), engineering (automata), and mathematics (information theory); and the author is a theoretical physicist of distinction. On the other hand, some of the biological documentation is clearly not pertinent, whereas some very pertinent biological information is not mentioned. One cannot take very kindly to vast claims based on limited biological knowledge. However, since the book has obviously been prepared with great care in certain respects, and since some portions of the argument do not depend on the validity of the biological documentation, a thorough analysis is desirable.

Old vitalist claims were based on the contention that some biological functions, such as self-reproduction, self-organization under varying conditions, and learning by trial and error, are specific to living things. Cybernetics has shown that this is not so; automata can be built or at least conceived which will perform the equivalent of any biological function. However, Elsasser points out that the fact that automata *can* do anything living organisms can do does not prove that living organisms do things in the same way as automata. Specifically, the mechanisms for memory in living organisms are very different from anything an electronic engineer would design, in that the germ cells are very small considering the enormous amounts of information they store; the memory functions which maintain the stability of adult organisms are vested in soft tissues and not in records maintained in the hard structures, where an engineer would place them; and, finally, cerebral memory has not yet been associated with any particular memory organ of the brain. Elsasser suggests that the reason the expected organs of information storage are not found is that in living things information simply is not stored, or at least not in the physical sense.

This is a startling proposition. In the course of communication, information is often subjected to many transformations, but it is generally assumed that information transmitted is physically observable at every stage of the communication process even when it cannot be "read out," as in the case of genetic information in transit from parent to offspring or cerebral information between first experience and remembering. Elsasser points out that this conviction is not based on experience or even on logical necessity. He postulates that living things *can* process information by "biotonic" functions, without using "mechanistic" (i.e., physically observable) storage, and proceeds to show that this is compatible with the laws of physics.

The argument is based on probability. Consider, for example, genetic information carried in the germ cell, presumably represented as specification of molecular structures. Given the number of molecular building units within a germ cell, the number of possible specifications is immense, larger by several orders of magnitude than the number of existing species. It is therefore possible that the genetic information characteristic of any given species is compatible with a vast variety of molecular specifications of germ cells. If there is actually great variety of molecular configurations, then the elaboration of relatively uniform characteristics from a huge variety would have to be done by biotonic functions. Such a process is beyond detailed analysis: the complete study of the molecular constitution of a given cell would entail more measuring operations than the cell could tolerate without being destroyed; and the derivation of a composite picture from different individuals is impractical because individuals are thought to differ on the molecular level, and only a

minute fraction of all individuals could be investigated. Under these circumstances, no physical observation could disprove the hypothesis of a biotonic, nonmechanistic information transmission. Biotonic functioning violates Newtonian causality. It is, however, compatible with a more general concept of causality based on succession in time occurring lawfully, without restrictions as to the nature of the law involved.

Logical compatibility is one thing, biological plausibility another. The picture of an immense variety of dynamically active molecular configurations is based on descriptions of intermediate metabolism. In this case, energy barriers separating the various compounds tend to be low, hence there is no great stability and much dynamic activity and—taking the system as a whole—a great variety of possible molecular configurations. However, one of the main functions of intermediate metabolism is precisely that of adjusting the distribution of substances within the cell independently of the concentrations of substances entering; in this sense intermediate metabolism is admirably suited to wipe out information. On the other hand, from molecular genetics a model of information storage based on macromolecules has been evolved which is strictly mechanistic and looks highly plausible. The macromolecules that are supposed to preserve information are stable, and the well-known ones exhibit little variety. DNA is subject to little turnover, and where turnover does occur, it hardly affects the ordering of monomers which preserves information. Furthermore, genetic information can be preserved with little or no metabolic activity in crystallized viruses, in spores cooled almost to absolute zero, and in deep-frozen cells; that is, under conditions incompatible with biotonic functioning.

It must not be forgotten, however, that molecular genetics is still largely a matter of hypothesis. Elsasser claims that no set of physical measurements on a horse's sperm could ever reveal that it will give rise to a horse. Today's molecular geneticists believe that a nucleotide sequence exists which unambiguously indicates "horse," but they do not expect that this sequence will become known in the near future. Still more remote is the possibility of a detailed analysis of how genetic information is actuated to determine every single trait. Finally, the problem of defining the characteristics of an unknown species from its nucleotide sequence may be basically impossible to solve. In this situation, consideration of the function of the genome as a whole must be either postponed indefinitely or deliberately based on incomplete knowledge; indeed, one may choose to disregard all mechanisms involved. There is but a tenuous boundary between mechanisms which could in principle be determined but which are deliberately disregarded, and mechanisms which are by their nature not capable of observation. Elsasser points out that much progress in physical theory has been made by first realizing that some phenomena, whose existence is generally assumed, continue to evade observation, and subsequently considering them as nonexistent. It may well be that some of the postulates of molecular biology should be abandoned on this basis.

Elsasser's book is stimulating and contains valuable passages. However, the validity of its central proposition is highly questionable in view of recent biological developments. One regrets sincerely that the author, while taking great care with the physical and epistemological aspects of his argument, did not penetrate a little deeper into contemporary biological thought.

HENRY QUASTLER

FOUNDATIONS OF INFORMATION THEORY.
By Amiel Feinstein. McGraw-Hill Book Company, New York, Toronto, and London. $6.50. x + 137 pp.; ill. 1958.

Information theory develops in several directions: there is a progressive elaboration of the mathematical apparatus, a widening of the domain of application, and an increasing rigor and solidifying of the mathematical foundations. Feinstein's book belongs in the third category. His first contribution to information theory was an improved statement of the basic "coding theorem," and this theorem occupies the central portion of the book. It is preceded by a careful exposition of the mathematical foundations of information theory, and is followed by a discussion of the most fundamental coding problems. The emphasis throughout is on concise but rigorous exposition. The biologist who wishes a rough and intuitive grasp of what information theory does and what it might contribute to biology will find the book rather hard going, and possibly unrewarding. To the serious student of information theory, it is a valuable reference work.

HENRY QUASTLER

SURVEY OF BIOLOGICAL PROGRESS, *Vol. III.*
Edited by Bentley Glass. Academic Press, New York. $7.50. 332 pp.; ill. 1957.

In this third volume of the well-known series started by George S. Avery Jr., the new editor, Bentley Glass, has collected six reviews of certain interest to different categories of biologists. All are authoritatively written and in a language understandable also to non-specialists, but since the authors of each report are specialists of great reputation, no single biologist can uniformly judge these six reviews. The first paper is by Jane M. Oppenheimer, who in little more than 30 pages summarizes the Embryological Concepts in the Twentieth Century. This is followed

by about 50 pages by David D. Keck on Trends in Systematic Botany; thereafter M. J. D. White uses less than 30 pages to present Some General Problems of Chromosomal Evolution and Speciation in Animals. The latter part of the volume is taken up by a paper on Chemoreception and the Behavior of Insects, by V. G. Dethier, another On the Mechanisms of Action of Hormones on Cells, by Rachmiel Levine, and last a review of Respiration and Cellular Work and the Regulation of the Respiration Rate in Plants, by George G. Laties.

It would be preposterous to try to evaluate these papers outside the special field of the reviewer, but there can be no doubt that all the treatments are of the same general quality. All the reviews are certainly clear and correct as far as they reach, and it is pleasant to see how much these authors have been able to put on so few pages, since all of them treat subjects of a size appropriate for at least a volume as thick as this entire book. Naturally, all review their subjects from their own points of view, biased as they always must be, but if the less initiated reader does not become interested in finding more information by aid of the selected bibliography referred to, that is not the fault of the writers.

If any remark is to be made on the reviews in general, it must of necessity be that the bibliographies include almost entirely English and American references, so that even unimportant papers published in the New World are mentioned while foreign papers, if written in another language, are almost absent. This may not be equally true for all the reviews, but it is certainly shocking to see that out of almost 250 references in the good paper on systematic botany by Keck, only 11 are in German, 5 in French, and 1 each in Portuguese and Latin. There may have been a valid excuse for excluding the very important Russian books and papers on this very subject published during the period covered by the review, but it is a great negligence to omit those many papers, fundamental to the present trend in systematic botany, which have been printed in the German language, and there is good reason to regret that although the list of references is filled with the names of even the smallest prophets from this side of the Atlantic, names of the present great European men of this science are absent, if their contributions were in German. Undoubtedly, the review would not have been impaired by mentioning Erwin Baur, O. Hagerup, E. Hultén, E. Irmscher, I. P. Lotsy, R. Mansfeld, H. Meusel, K. J. Pangalo, W. Rothmaler, E. N. Sinskaja, R. Soé, and W. Zimmermann, and these are but a few who have contributed greatly to the strong modern trends in systematic botany which started in Europe years ago, although their influence did not ooze through the thick walls of American conservatism until lately. There is reason to believe that this criticism should not be directed at this review alone, but also to the other papers, since it is hardly likely that German-writing scientists failed to contribute to all the problems under discussion in a considerably higher degree than the bibliographies seem to indicate.

Nonetheless, in conclusion it must be said that although these reviews necessarily must be limited in scope and coverage, they are authoritative and clear, and likely to be of great assistance in adding to the knowledge of students and widening the interests of scientists. The volume—and the entire series—should not only be placed in all biological institutional libraries, but should also be found on the shelves of individual biologists.

ÅSKELL LÖVE

THE HARVEY LECTURES. *Delivered under the Auspices of the Harvey Society of New York 1957–1958 under the Patronage of the New York Academy of Medicine. Series 53.*

Academic Press, New York and London. $7.50. xvi + 254 pp.; ill. 1959.

The Harvey Society Lecture series cover progress in the medical and biological sciences through the writings of individuals who have developed, over long periods, research programs on topics of wide biological application. The usually informal discussions serve as excellent introductions to diverse important areas of biological and medical research.

The new series opens with an extensive analysis of epidemiological data, collected by J. H. Dingel and associates over a period of 9½ years. Although the data deal with a highly selected group of American families, several suggestive concepts of general nature arise from the careful study (e.g., there is a year-to-year consistency of occurrence of new cases of respiratory infection during the six winter months in spite of great year-to-year fluctuations in the contributions of specific agents, such as influenza virus). One omission from most accounts of population genetics, so preoccupied with the mathematical approach, is a consideration of the roles of infectious agents in speciation and evolution. However, microbiology, except for a few instances pointed out elsewhere by Dubos, Burnet, and others, has provided but scanty data to whet evolutionary speculation. F. Fenner's review of the population dynamics of host-parasite interactions following the 1950 escape of myxomatosis virus into a vast susceptible rabbit population in Australia should help shape some thought along these lines. One factor which makes the study of great usefulness is the concomitant analysis of virus and host strains in the laboratory, supplementing the field and population studies to a degree not heretofore possible.

H. Fraenkel-Conrat summarizes some recent experiments concerned with the effect of various treatments on ribonucleic acid extracted from tobacco

mosaic virus. He also mentions advances in analyses on the structure of the protein subunits which, in the intact virus, interact with and stabilize the genetically active ribonucleic acid. His statement that "this rod-shaped virus is a treasure-trove of chemical surprises" is indeed borne out by the results he reviews. J. Lederberg neatly summarizes, in historical perspective, the main advances in "bacterial reproduction" (a topic which turns out to be recombination by conjugation of enteric bacteria). A. Kornberg follows with a description of a beautiful series of experiments now unravelling the intricacies of the chemistry of deoxyribose nucleic acid synthesis. These studies, along with recent physicochemical and genetic experiments, take genetic materials from the realm of nebulous "self-duplicating" structures into the area of enzyme-mediated biosyntheses. Significant experiments on the cellular site of antibody formation are surveyed by A. H. Coons. Extracted from the voluminous immunological literature, these few fundamental studies will be of interest to the increasing number of biologists and chemists who are joining the currently revived interest in the mechanism of formation of antibody protein. D. Mazia takes the reader from the beginning into his most recent results on the isolation and characterization of the mitotic apparatus and its two major protein components. His lucid style allows one to follow his thought during development of superior isolation techniques and his current concept of the mitotic apparatus, its origin, structure, and mode of functioning. Two papers on roentgenological and pathological changes in some diseases of the lung (J. Gough) and on extracorporeal maintenance of cardiorespiratory functions (J. H. Gibbon, Jr.) conclude this fine new volume of Harvey Lectures.

PHILIP E. HARTMAN

INSIDE THE LIVING CELL. *Some Secrets of Life.* *By J. A. V. Butler. Basic Books, New York.* $3.50. 174 pp.; ill. 1959.

The need for books which can interpret scientific discoveries to an attentive but essentially untrained audience is urgent, for the breach between the scientist and the general public is widening at an increasing and alarming rate. In this and his earlier books, J. A. V. Butler has shown himself to be as equal to this task as he is to his more sophisticated appointment as research biochemist in the Chester Beatty Institute for Cancer Research in London. In this sense he keeps alive a great English tradition that has produced such fine popular writers as Julian Huxley, Medawar, and Darlington, to mention but a few. It is to be regretted that the American scientist does not, as a rule, feel such writing to be of equal importance.

In the present book, Butler gives an excellent account of the structure and function of the living cell. Although gaps exist and the account is strongly oriented toward biochemistry, the subject matter is up to date, well organized, and skilfully handled. Simplification is accomplished by good writing and not by avoiding complicated but necessary information. The book is addressed to a lay audience, but every student of biology can profit from it; it is a worthy addition to the list of good books in general science.

C. P. SWANSON

BIOLOGY: HISTORY AND BIOGRAPHY

A HISTORY OF CYTOLOGY. *By Arthur Hughes. Abelard-Schuman, London and New York.* $5.00. x + 158 pp. + 12 pl. 1959.

The science of cytology is a small, though important, segment of biology and a still smaller one of our total human experience, but to those of us who labor in its vineyards it is a world of orderly yet changing beauty of architecture and function, and one capable of yielding, quite apart from its scientific aspects, an immense aesthetic pleasure. A beautiful preparation can excite me as much as a fine symphony, poem, or novel. The microscope, of course, made possible the study of cells, but since concepts are as much a part of it as observations, cytology is studied with personalities, discoveries and re-discoveries, interpretations and re-interpretations.

Arthur Hughes, a thoroughly capable cytologist himself, has now undertaken to write a history of the subject. Cytology is, as he points out, intermeshed with many other branches of biology, yet retains a disciplinary identity of its own. It began with the invention of the microscope, blossomed into maturity during the last quarter-century of the 1800's in Europe, and today has an impact on many adjacent fields of biological and medical inquiry.

In his presentation, Hughes begins with the development of microscopical observations, recognition of the cell, division of the cell and nucleus, the role of the nucleus in heredity, study of the cytoplasm, and, lastly, cellular theory in general biology. The volume is a scholarly piece of work, lucidly written, and thoroughly documented. With so few historical volumes available in biology, this one is particularly welcome. Even though its subject is a limited one, "a greater awareness of historical perspective . . . , would be a welcome improvement in cytological literature [and this reviewer believes, in our students as well]. It might help to dispel the illusion too often implicit in reports of fresh discoveries, that the final secrets are nearly within reach." Or, if additional justification is needed, the following statement by F. J. Cole is pertinent: "Much con-

fusion of thought would be avoided if the historical approach to scientific learning were permitted to occupy a more prominent place in the curriculum. The boundless optimism of the research worker would be suitably curbed, and the prodigal exploitation of popular and ambitious speculations would be reduced." Possibly this will provide an incentive for further historical volumes of a similar sort.

C. P. SWANSON

THE AUTOBIOGRAPHY OF CHARLES DARWIN AND SELECTED LETTERS.

Edited by Francis Darwin. Dover Publications, New York. $1.65 (paper). xiv + 365 pp. 1959.

The publisher states: "This new Dover edition . . . is an unabridged and unaltered republication of the work first published in 1892 in the United States by D. Appleton and Company under the title, *CHARLES DARWIN, His Life Told in an Autobiographical Chapter and in a Selected Series of his Published Letters.*" As such, it is an abridgment of the *Life and Letters of Charles Darwin*, edited by his son Francis Darwin, and is not to be confused with the recently published "*complete*" *Autobiography*, in which certain matters deleted at the request of the family have been published for the first time. This inexpensive volume will nevertheless be valuable to students of Darwin in this centennial year if they do not already possess the *Life and Letters* in the two-volume edition.

COLLECTED PAPERS. *Five Volumes.*

By Sigmund Freud; Vols. 1–4 edited by Ernest Jones; Vol. 5 edited by Ernest Jones and James Strachey. Basic Books, New York. $25.00 (the set). Vol. 1, 359 pp.; Vol. 2, 404 pp.; Vol. 3, 607 pp.; ill; Vol. 4, 508 pp.; ill.; Vol. 5, 396 pp. 1959.

This set is a new edition of a series of books issued from 1924 to 1950 under several editors. Most of Freud's shorter papers are included. The subject matter varies greatly. The first paper is an obituary of Charcot; the last is a paper published posthumously concerning a revision of the *Outline of Psycho-Analysis*. Clinical and theoretical materials are both presented in various other papers. Freud's thoughts on sculpture, painting, Shakespeare, war, civilization, and the interpretation of dreams are all included. The famous "Hans" paper, a slightly polemical early history of the psychoanalytical movement, and a portion of Freud's psychoanalysis of himself add to the interest that this collection will have for the lay reader.

The papers are presented in a historical sequence and serve to show how Freud's ideas on various phases of psychoanalysis changed during his life. They start with the original paper on psychoanalysis published in 1893 in collaboration with Brewer and end with the papers that Freud was working on when he died. We see Freud's growing interest in the application of the psychoanalytical concepts, which he displays somewhat hesitantly at first, even to the extent of originally publishing one paper anonymously.

There is some unevenness in style, due in part to differences in editors between the various volumes. Some small errors in translation have crept through which suggest that for key passages the reader will do well if he compares the translation with the German original.

MYRON L. WOLBARSHT

BIOGRAPHICAL MEMOIRS. *Volume XXXI.*

The National Academy of Sciences of the United States of America; Columbia University Press, New York. $5.00. viii + 396 pp. + 15 pl. 1958.

This is the latest collection of biographical essays and portraits of recently deceased members of the Academy. Of the 15 memoirs, biologists will be especially interested in those of embryologist, Edwin G. Conklin; malacologist and paleontologist, William H. Dall; psychologist, Walter S. Hunter; plant pathologist, Lewis R. Jones; anthropologist, Ralph Linton; physical biochemist, Leonor Michaelis; physician and biochemist, John P. Peters; and enzymologist, James B. Sumner.

FRANK ERK

THE YOUNG NATURALIST

SCIENCE IN YOUR OWN BACK YARD.

By Elizabeth K. Cooper; illustration by the author. Harcourt, Brace & Company, New York. $3.00. 192 pp.; ill. 1958.

Science in Your Own Backyard is an interesting little book which serves as an excellent introduction for the child to the natural sciences. Elizabeth K. Cooper, Director of Elementary Education for the Santa Monica City Schools and Coordinator of Teacher Training for the University of California, describes how to become an explorer. Within your own neighborhood you can discover something about the soil and rocks, grasses and flowers, insects, snakes, birds and other animals, the stars, and the weather. Indeed all the ingredients for being a young scientist are outlined, including what to do, what to observe, and even the proper questions to ask! Many simple and interesting experiments are described which can be done with easily available materials. For example, using aluminum-foil pans and trays (from

TV dinners, chicken pies, etc.), peanut butter and mayonnaise jars, tin cans, glue, scotch tape, crayons, notebook paper, and many other odds and ends, you can set up an earthworm farm, grow grasses and seedlings, collect spider webs, build a feeding station for birds, maintain a complete weather station (actually determine air pressure, humidity, precipitation, wind direction, and speed), and learn the constellations through a cardboard telescope.

The book really seems to be an autobiography of the author's childhood. Its value is mainly as a guide for parents, to kindle in their children the scientific approach and an interest in natural phenomena. Unfortunately, those parents who will be interested in the book most probably have already done this and those children old enough and wise enough to read the book on their own are very probably the children of such people, or have in some other way already acquired the knack. The educational problem is, how to reach those children who could benefit from the book most? For the experiments are novel and refreshing and serve as a good laboratory manual for everybody's backyard.

SHERWOOD M. REICHARD

ECOLOGY AND NATURAL HISTORY

ZOOGEOGRAPHY. *A Symposium presented on August 26–27, at the Stanford University Joint Meeting of the AIBS and the Pacific Division of the AAAS and a Symposium presented on December 28, 1957 at the Indianapolis Meeting of the AAAS.*

Edited by Carl L. Hubbs. Publ. 51 of the American Association for the Advancement of Science, Washington. $12.00; prepaid to AAAS Members $10.50. x + 509 pp.; ill. 1958.

This volume contains the published proceedings of two symposia sponsored by the American Association for the Advancement of Science. The first two-thirds consists of 14 papers presented at a symposium entitled "The Origins and Affinities of the Land and Freshwater Fauna of Western North America." The last third consists of 3 papers presented at a symposium entitled "Geographic Distribution of Contemporary Organisms." There doesn't seem to be any reason for having the book in two parts except that the papers were given on two occasions. Most of the book is concerned with the zoogeography of western North America, one chapter in the second part with the southern United States, and another in this part with North America as a whole. The unity of the volume lies in its emphasis on North America, and it is therefore a pity that this was not indicated in the title, which is completely general and to some extent misleading.

The fragmentation of the subject by 16 specialist authors makes any general review difficult. This has however been excellently done by the editor himself in a final chapter entitled General Conclusion, where some pointed comments are made on most of the contributions. To some extent this chapter helps to tie together what otherwise appear to be disconnected contributions. I think the reader will go to this book not for any general light upon zoogeography but for special understanding in regard to the distribution of certain groups in the North American continent, and perhaps also for an approach to the study of zoogeography. The following groups are specially treated: freshwater invertebrates, insects, amphibia, reptiles, birds, and land mammals. Emphasis is away from the purely descriptive toward explanation of present distributions. A surprising amount of detail is included in some chapters on methods of studying and interpreting zoogeographical data, e.g., on criteria of the origin of groups. The worthwhileness of the volume is in the large amount of material published here for the first time. Some of the more general chapters cover the geology, past climate, and ecology of North America. The special significance of the western part of North America is illustrated by Peabody and Savage in their account of the Pacific Coast Range corridor formed in the Pliocene and early Pleistocene. However, the editor inclines to think that similar situations exist outside the Golden West and that "Zoogeographical gold is where you find it"! This being the case, perhaps this volume might be regarded as the first of a series which could be made on other regions, especially now that zoogeography is being welcomed back to the fold of zoology after a long period in a relative backwater.

L. C. BIRCH

ECOLOGICAL PROCESSES.

By Alan Mozley. H. K. Lewis & Co., London. 9s. xii + 68 pp. 1959.

The purpose of this book escapes one. It comments simply on ecological succession and on populations without showing clearly that processes do exist. Extensive quotations from various authors confuse the exposition, since these remarks are not clearly related to the extensive quotations from the author's other writings. The examples chosen deal primarily with snails, although some vertebrates are mentioned.

DAVID E. DAVIS

WORKBOOK FOR FIELD BIOLOGY AND ECOLOGY.

By Allen H. Benton and William E. Werner. Bur-

gess Publishing Company, Minneapolis. $2.75 (paper). x + 268 pp. 1957.

This "ring-bound" booklet contains a miscellaneous assortment of outlines of field techniques that may be used at the high school or junior college level. The outlines pertain to land and water communities, populations, and behavior. The exercises are simple and direct, but unfortunately they do not introduce the student to basic problems concerning variability, sampling error, or fluctuations.

DAVID E. DAVIS

A BIOGEOGRAPHY OF REPTILES AND AMPHIBIANS IN THE GÓMEZ FARÍAS REGION, TAMAULIPAS, MEXICO. *Misc. Publ. Mus. Zool. Univ. Mich., No. 101.*

By Paul S. Martin. University of Michigan, Ann Arbor. $1.50 (paper). iv + 102 pp. + 7 pl.; text ill. 1958.

This study was made at the edge of the American tropics. In a small area of southwestern Tamaulipas, in the Gulf drainage of Mexico, three important tropical plant formations—Cloud Forest, Tropical Evergreen Forest, and Tropical Deciduous Forest—have their northernmost outposts and interdigitate with temperate communities. As clearly as anywhere in North America, tropical and temperate biotas come together here. Martin's object was "to characterize zones in terms of vegetation first and then to define the degree of faunal fit." He found 94 species of terrestrial reptiles and amphibians, separable into three ecological groups: a humid montane group, comprising populations isolated in their insular mountain forest habitat; a dry woodland-Thorn Scrub group; and a lowland tropical group. Although hindered in his appraisal by the dearth of information on the structure and dynamics of tropical communities generally, Martin has made a sound and interesting contribution to the ecological biogeography of a unique region that is changing fast under saw and tractor.

ARCHIE CARR

LES MIGRATIONS DES OISEAUX.

By Jean Dorst. Payot, Paris. 1,500 fr. (paper). 419 pp.; ill. 1956.

Although there is no up-to-date book in English which covers the entire field of bird migrations, several good surveys of the subject have been published in recent years in other European languages. This latest book in the series contains very little original material, but nevertheless provides the most comprehensive summary of the basic publications on the subject. The bibliography is complete to 1953, with a few additional references dated as late as 1955.

Skilfully abstracting the vast mass of specific and regional literature, the author devotes nearly half the book (156 pp.) to an account of the migrations of representative species throughout the world. This includes valuable sections on the migration of tropical and oceanic species, but that on migration in Europe will probably be the most useful part of the book, at least to the American reader. Intensive banding and other field studies in Europe have revealed many interesting variations in migratory behavior, which are associated with the diversified local climate and are without parallel on this continent. The least satisfactory section is that on migration in North America, but this is chiefly due to the lack of published data: most of the information quoted was first published in 1915 and is badly in need of revision.

The remainder of the book contains chapters on early theories of migration, methods of study, height and speed of flight, influence of weather and topography, irregular migration, hibernation, physiology, navigation, and evolution of migration. Like much modern work on bird migration, these essays are most valuable when evaluating the results of experiments made under controlled conditions; the discussions of field studies do not always indicate the full complexity of the environmental factors which affect migrating birds. Moreover, even the best chapter—on the physiological regulation of the migration period—is concerned with little more than basic hypotheses, and is no substitute for the detailed contemporary review by D. S. Farner (in *Recent Studies in Avian Biology*, 1955).

Throughout the book the author warns against the acceptance of hasty generalizations, and repeatedly stresses the variability of migratory behavior. However, much of this diversity is already partly understood, for it can plausibly be related to variations in environmental factors which are known to influence bird migration. The book avoids such speculative ideas and presents no exciting new syntheses of the facts, but within these limits it is accurately and clearly written, and can be thoroughly recommended.

I. C. NISBET

LIFE HISTORIES OF CENTRAL AMERICAN BIRDS. *Families Fringillidae, Thraupidae, Icteridae, Parulidae, and Coerebidae. Pacific Coast Avifauna, No. 31.*

By Alexander F. Skutch; illustrated by Don R. Eckelberry. Cooper Ornithological Society, Berkeley. $10.00. 448 pp; ill. 1954.

No other ornithologist knows about the living Neotropical birds as well as Alexander Skutch. The volume of information that has issued from his pen is truly amazing. The present volume, including only 5 families and representing only a portion of Skutch's writings, attests to the tremendous amount of de-

voted work that he has accomplished. Observing birds in the tropics is not the easiest nor always the most comfortable occupation, yet the author has been able to amass details of life history on many species, the previous knowledge of which consisted of nothing more than their taxonomy. Forty-one species are treated in this volume, some naturally more intensively than others. A general summary of information is also provided for each family. Each species is illustrated by a line drawing by Don Eckelberry, and some photographs of habitats or nest sites are included. There is a beautifully colored frontispiece showing the more glamorous tanagers. Skutch not only has the ability to observe and to listen, but he also possesses a flair for writing, so that his accounts are a pleasure to read. This publication is, without doubt, a major contribution to ornithology.

HENRI C. SEIBERT

THE BREEDING BIOLOGY OF THE CHIMNEY SWIFT, *Chaetura pelagica* (LINNAEUS). *Bull. No. 368.*

By Richard B. Fischer. New York State Museum and Science Service, University of the State of New York, Albany. $1.25 (paper). 141 pp.; ill. 1958.

It is with admiration that this contribution is greeted, for it represents the culmination of a considerable number of patient observations that started in 1939. As a result, many aspects of chimney swift biology are revealed and explained and many former false statements are corrected. No morphological sexual difference was found, but clues were obtained from the behavior of the parents at the nest and with the young. The well known trio-flying, consisting of two males chasing a female, is considered to be display at its highest intensity. It is suggested that the V-ing flight trait is used to maintain the sexual bond, since it occurs most frequently in mated pairs. The detailed description of copulation at the nest site seems to dispel any notion that this activity occurs on the wing. Nest construction, nestling behavior, and parental care are described carefully. Sticks are carried in the bill by both sexes, and construction of the nest continues even after the first egg has been laid. The characteristic semicircle of saliva on the wall above the nest is added by the incubating bird. Data on clutch size, incubation period, banding returns, age survival, growth of young, and fledging, among other categories, are presented. Very informative photographs further help to make this report one of the best yet on the home life of this species.

HENRI C. SEIBERT

ELEPHANTS. *A Short Account of their Natural History, Evolution and Influence on Mankind.*

By Richard Carrington; illustrations by Maurice Wilson and others. Basic Books, New York. $5.00. 272 pp. + 24 pl.; text ill. 1959.

This is a popular science book about elephants which should provide enjoyable reading for anyone interested in these great beasts. The author has divided the book into three main sections. The first of these deals with the natural history of elephants, some of the information having been derived from observations of zoos and circus veterinarians, but most of which has been obtained from the accounts of British colonial officials with a naturalist proclivity. The distribution of the two African subspecies and of the Asian species, together with what information seemed to be available concerning their physiology and life cycle, is presented. The second—and probably the best section—is concerned with the evolution of elephants. An abridged version of Osborn's reconstruction of the evolution of the Proboscidia is given, in a very readable style. While the scientific names in this section might overwhelm the reader, they are reduced in number as much as the nature of the subject permits. The last section concerns the elephant's relations with man, i.e., in folklore and magic, as hunted by man, as a working animal, and as a performer. The last chapter of this section is something of a prospectus of the elephant's chances for long-term survival in the modern world. It is gratifying to note that the author feels that the elephant is holding its own.

This is an interestingly written and adequately illustrated book which should serve well in introducing a newly interested person to the subject. While the bibliography is not exhaustive, it contains key references that will allow the deeply interested person to follow the subject more extensively.

RONALD R. COWDEN

THE BLACK FENS.

By A. K. Astbury. The Golden Head Press, Cambridge; [W. Heffer & Sons, Cambridge]. 42s. xii + 217 pp. + 55 pl. + 1 folded map; text ill. 1958.

The earth's surface has been so extensively and profoundly transfigured by man that the average imagination has difficulty in recreating the conditions of even a few generations ago. In Europe, in particular, the natural cover of forest has long since vanished and only the more inaccessible and intractable areas of mountain, moorland, and fen have withstood the changes. But the pressure of economy in recent times has begun to bring these also under harness. Thus, hundreds of tons of peat daily feed the great power plants of southern Ireland, and all over the British Isles the sombre mountain moorlands are surrendering to the green of exotic conifers. For a much longer time, and because of the richness of their soils, the vast fenlands

of eastern England have been under cultivation. It is of these, of the Black Fens surrounding the historic Isle of Ely, that Astbury writes. His first chapter immediately isolates the essential atmosphere of the area by emphasizing the contrast between the open, black fen and the huddle of houses in the friendly, picturesque fen towns such as Ely, a friendliness especially felt at night when the lighted windows hearten and welcome the traveler across the cool, dark flatlands.

The Black Fens cover an area of some 1300 square miles stretching from Cambridge in the south to Lincoln in the north, and from Peterborough in the west to Lakenhead in the east. Early writers described them as "hideous fens of huge bigness," but today they are refined under ditch, dyke, and farm, and the casual visitor, acclimatized to England's matured landscape, criss-crossed with friendly and rich hedgerows, will be astonished only at finding within the same country such vast, unbroken plains, flat as the sea, and only here and there above sea-level.

For an entry to this remarkable area Astbury recommends a visit from the village of Reach, leaving it on an autumn afternoon for Ely. But it is equally enchanting to be introduced to the area from Cambridge, going again to Ely this time by way of Wicken Fen. In early August the fen—now a protected reserve—is at its best, full and ripe. Cambridge is indeed fortunate to have this ground for its students so close at hand. Moreover, what other flatland has such an enrichment as the spires of Ely cathedral to bestow inspiration like a benediction upon the landscape?

Astbury describes fenland life and its problems from the viewpoint of farmer, house-builder, road and railway constructor, and, most important of all, drainage engineer, upon whom the continuance of the area depends. The farmer has a prosperous association with the rich soils, but it is one that is periodically made hazardous by serious fen fires and, even worse, by the soil or dust storms which have become frequent in the past fifty years. These storms strip whole fields of their crops, scatter the valuable surface soils, choke canals and ditches, and provide the worst examples of soil erosion in England. The builder experiences frustration in his attempts to lay stable foundations on a wasting and shrinking peat floor, and apart from costly excavation he must rest his buildings on concrete rafts or on wooden piles. The road constructor has the same problem but on a vaster scale, and must be resigned to the fact that in fifty years his road will certainly have subsided and may sag between ridges of clay beneath the peat, or lie as it were suspended between the bridges resting on the more permanent silt levees or "rodhams" on the rivers. The drainage engineer faces the worst problem of all, for his rivers must flow across the subsiding fenland—already below sea-level, from an unsinking highland to an unsinking sea. To do this they must be carried across the saucer of the fen on raised embankments which continually threaten to collapse. Obviously the Black Fens challenge the creative talents of its peoples, and Astbury succeeds in conveying to the reader the intimate texture of that challenge.

If there is criticism of this book, it is first that there is a certain imperfection in Astbury's history of fenland formation. In explaining the marine submergence of the area in Neolithic and Roman British times, Astbury recalls the complex interactions in the North Sea region between a rising sea-level and a downward crustal movement, both a consequence of the retreat of the last ice sheets. His account is generally correct, but he errs in claiming that when the Ice Age ended, the North Sea bed was some 200 feet above its present level. The sea bed was indeed dry land or marsh-land for some thousands of years, but this was due to low sea-levels rather than high land-levels. The whole history of the fenland area in terms of land and sea movements has been the subject of thorough researches by Cambridge scholars, and Astbury would have been wise had he followed their results closely and supplemented the word picture of the fenland history—which at best will be complex—with a summarizing table or diagram.

The second criticism regards the failure to bring to a focus in any one chapter of the book the factors which molded the birth and growth of the major fen towns, such as Ely, Marc, and Peterborough. Information of this character is scattered throughout the book and is troublesome to collect. This failure to delineate clearly the historical, as against the prehistoric, evolution of the region detracts considerably from the wholeness which the book might otherwise have possessed.

The text has many excellent photographs of fenland vistas, dust storms, and houses, while there are some good aerial views showing how old and abandoned waterways and river beds may still be traced, although they may be almost invisible to the ground observer. Perhaps a visitor buying the book as a souvenir of a visit to the region will miss the fact that there are no illustrations of the towns, and, worse than this, none of Ely cathedral with its celebrated octagonal lantern. The book ends with a well-drawn map and a replete index wherein one may browse happily among such wonderful place names as Prickwillow, Caldecote Fen, Funtham's Dyke, Swasedale Drove, and Wimbotsham parish!

M. E. S. Morrison & Áskell Löve

CONSERVATION OF NATURAL RESOURCES. *Second Edition.*

Edited by Guy-Harold Smith. John Wiley & Sons, New York; Chapman & Hall, London. $8.50. x + 474 pp.; ill. 1958.

This second edition of a familiar textbook, although reduced from 552 to 474 pages, is still, like the first, considerable in bulk and variety of content. Five new authors have replaced 6 of the original 20, illustrations have been revamped, and chapters largely rewritten. A new one on Economics and Conservation has been added to the two valuable historical chapters with which the book begins. Fortunately this new chapter takes note of the importance of social objectives, a fundamental matter that is obscured in some of the later sections in which it should have been faced squarely. Many of the treatments of individual resources are excellent, specific both in appraisal and technical matters, clear and concise in presentation, with good summaries at the end. This is true, for example, of the discussion of wildlife, which may be cited as a model. Unfortunately, there are also chapters which are complicated and diffuse, with no attempt to draw together their content in concluding paragraphs.

Least satisfactory are the more general chapters —those which should be most lucid and well organized if they are to clarify the thinking of future citizens. While there is a gratifying absence of the familiar hoopla about the unlimited promises of technology, there is no attempt at basic ecological analysis. One is somehow left with the impression that the problem of natural resources is essentially operational and legislative, rather than one of cultural values, ethical and aesthetic, based upon an understanding of natural law.

There is available plenty of evidence from such diverse fields as anthropology, demography, population biology, and physical science to support this latter position. It can and should be marshalled to to give a framework of principle in dealing with man and the resources that sustain him.

The book is at its best as a geography of the natural resources of the United States. It is not, except for penetrating comments by individual specialists, a philosophical analysis of the resource problem. Its usefulness must be judged accordingly. In the hands of a teacher whose own thought upon the subject has matured, it can be of considerable value. With instructors less well qualified it is likely to produce confusion on both sides of the lectern. Of that we have enough. There are still too many of us who think of resources in terms of such simple alternatives as "why worry" or "there ought to be a law."

Apart from these important considerations, the success of the editor in securing the talents of a number of very able contributors deserves commendation. Only those who have tried to produce so ambitious a symposial volume can appreciate the persistence and labor that is required.

PAUL B. SEARS

EVOLUTION

THE IMMENSE JOURNEY.

By Loren Eiseley. Random House, New York. $3.50. 210 pp. 1957.

Parts of this volume appeared in print as early as 1946, and it may well be that the centenary of Darwin's *Origin of Species* has prompted its reprinting, thus making it a companion of other books on Darwinism which have appeared and will continue to do so throughout the year. Be that as it may, I can only be grateful that it has been done, for it is one of the finest accounts extant of the whole panorama of evolution. Simply written, but with a literary elegance attained by few of our contemporary scientists, it possesses that surety of touch and breadth of outlook that is the mark of one who not only knows whereof he speaks, but also knows he has a story worth the telling.

The Immense Journey is the story of the origin of man from Pooh Bah's ancestral "protoplasmal primordial atomic globule" to the man of the future. But it is also more than this, for it is as well a serendipitous journey, full of unexpected delights. As a botanist, I thought the chapter, How Flowers Changed the World, a rare treat, and my students, skeptical beings though they are, thought so too. At a time when the reappraisal of Darwinism is the subject of the hour, there will be no more appropriate reaffirmation of its essential truth than that which Loren Eiseley offers in this modest, but compellingly persuasive, book. Would that there were more volumes like it.

C. P. SWANSON

THE MOLECULAR BASIS OF EVOLUTION.

By Christian B. Anfinsen. John Wiley & Sons, New York; Chapman & Hall, London. $7.00. xiv + 228 pp.; ill. 1959.

Astounding progress has recently been made in fine structure analyses of nucleic acids and proteins. Comparable progress in genetics has prepared the advocates of this science, first for a profitable union with those interested in the chemical and physical properties and structure of nucleic acids, and now, with specialists on the proteins. Anfinsen, one of

the latter, succinctly summarizes the recent developments leading to an integration of these disciplines. He points out, in simple terms, some basic problems that only such unions of effort are capable of answering. His book, easy to read and "written for pleasure, with the desire for self-enlightenment as the major stimulus," should generate excitement, from college students of genetics and chemistry (for whom the book makes a very good supplement to standard genetics textbooks, all very weak in "molecular biology") to senior research workers in many different fields of the biological and physical sciences. A survey of recent Darwin centennial symposia and volumes indicates that a majority of those scientists whose "province" has been the consideration of evolutionary processes could well join the number of those spending profitable time in reading this book. Although the contents (and currently available data!) fall far short of fulfilling the ambitious title Anfinsen has given this little volume, the author points out enough promising possibilities to engender profitable ideas in even the most thoroughly ingrained naturalist.

The book opens with a cursory, yet clearly worded, treatment of elementary evolutionary and genetic principles. To simplify his presentation, Anfinsen avoids specialized jargon to the limits of possibility. This sometimes leads to errors (such as the improper use of "chromosomal strand" on pp. 21 and 28, or of "two rounds of meiosis" on p. 33 and in figure 19). There are a few other errors which could readily have been corrected had the manuscript been edited by a trained geneticist (e.g., p. 23 refers to an absence of chiasma in male *Drosophila*).

The next chapter, on the chemical nature of the genetic material, is possibly the weakest chapter of the book, although this subject would seem prime for a general, simple, and up-to-date review of the type Anfinsen handles so well in other chapters. Very good chapters follow on bacteriophages and their fine structure genetics, a topic to which the author later returns in a discussion of gene-protein relationships. Crystal-clear introductions to the subjects of protein structure, species differences in relation to chemical modifications of protein structure, and the relation of structure to biological function comprise the major part of the book. Anfinsen points out: "Except for those rare phenomena in biology which are purely physical, the 'aliveness' of cells is basically the summation of enzyme catalysis and its regulation . . . . the most likely approach to the understanding of cellular behavior lies in the study of structure and function of protein molecules." To these may be added, as Anfinsen implies by the topics selected for coverage, how protein molecules are made.

The book has an abundance of well-conceived and clear figures, many of them garnered from the recent literature, a large number of others original. Unfortunately, some of those redrawn from the literature lack complete legends (e.g., figures 14 and 26) or have incorporated mistakes (compare p. 41 with figure 43; figure 99). Such errors might have been prevented by a brief consultation with those whose work was cited.

PHILIP S. HARTMAN

FOSSIL AMPHIBIANS AND REPTILES. *Brit. Mus. (nat. Hist).*

By W. E. Swinton. The British Museum (Natural History), London. 5s. (paper). x + 118 pp. + 17 pl.; text ill. 1958.

Since the end of World War II the British Museum of Natural History has published an excellent series of new guidebooks which are essentially introductory treatises on the various subjects displayed in its galleries. This volume on fossil amphibians and reptiles, now in a revised edition, provides a brief illustrated survey of the extinct cold-blooded tetrapods. The more frequent references to Old World rather than American examples provides variety from the usual popular account and serves as a reminder of the faunal differences between the continents. A glossary, table of geologic time, and brief classification of the amphibians and reptiles provide basic information the novice would require in order to understand the text.

Well-preserved plesiosaurs and ichthyosaurs are among the most common fossil vertebrates of Great Britain, and these, along with the dinosaurs, have been treated more fully and in a more interesting fashion than other groups. By contrast, the 5 pages devoted to mammal-like reptiles scarcely do justice to the fine collections of these significant reptiles at the British Museum. Indeed, this chapter is somewhat confusing and in places contrary to currently accepted views on therapsid relationships. The statement that the first mammals are found in mid-Jurassic rocks leaves the critical reader wondering how Swinton classifies the recently discovered Rhaetic and early Liassic fossils which are currently regarded as the earliest mammals.

Restorations of extinct animals form an important part of any general treatment of paleontology. A series of black-and-white reconstructions of important fossil reptiles and amphibians by Maurice Wilson enhance this handbook and are particularly interesting for the oriental style of the backgrounds, plants, and even of some of the animals. *Cynognathus*, as restored, suggests a Japanese dragon more than a near mammal. Some extremely old illustrations of imperfect specimens, some with little

purpose, contrast unfavorably with many excellent photographs and modern drawings. The figure on p. 27 illustrating various types of reptile skulls is copied with only slight alterations and without acknowledgment from one in E. H. Colbert's *Dinosaur Book*.

A curious lapse appears in the attribution of the parapsid type of reptile skull to the Squamata (the view held by the late S. W. Williston, now generally abandoned). This statement is not in accord with the tabular classification at the end of the volume, which places the Squamata in the diapsid subclass Lepidosauria.

Notwithstanding such minor shortcomings, some of which are inevitable in any book, this manual is worthy of the fine series in which it appears. It can be recommended to anyone seeking a sound introduction to paleoherpetology.

JOSEPH T. GREGORY

THE GEOLOGY AND PALEONTOLOGY OF THE ELK MOUNTAIN AND TABERANCLE BUTTE AREA, WYOMING. *Bull. Amer. Mus. nat. Hist., Vol. 117, Art. 3.*

By Paul O. McGrew; with the collaboration of Jack E. Berman and 4 contributors. American Museum of Natural History, New York. $1.50 (paper). Pp. 117–176 + 7 pl. + 1 folded map; text ill. 1959.

GENETICS AND CYTOLOGY

BIBLIOGRAPHY ON THE GENETICS OF DROSOPHILA. *Ind. Univ. Publ., No. 21. Part Three.*

By Irwin H. Herskowitz. Indiana University Press, Bloomington. $2.50 (paper). x + 296 pp. 1958.

A continuation of the bibliographic work begun by H. J. Muller (Part One) and continued by Irwin Herskowitz (Part Two). The present volume includes listings of more than 3100 titles for the period 1951 through 1956, an average of 517 per annum. During the period of 15 years 1925–1938 inclusive there were about 173 papers published per annum in this branch of genetics; and during the period of 12 years 1939–1950 inclusive there were 237 per annum. The increase of more than 100 per cent during the most recent period, as compared with the immediately preceding period, is greater than the increase between the first and second periods covered by the *Drosophila Bibliography*. This evidence of scientific productivity should be ample answer to those questioning spirits in other fields who have been heard to state that the great period of *Drosophila* work lay in the period 1910 to 1930, that everything is now known that is ever likely to be learned from future studies with this species group, and that Drosophila genetics is quiescent if not dead. The extraordinary advantages of working with an organism genetically so well-known as *Drosophila melanogaster* seem borne out by these statistics. It would be interesting to have comparative figures, say, for the genetics of bacteria.

Titles are arranged alphabetically by author, with co-author cross-references interpolated. There is also a title index in three parts: a general index by subjects, including indexing by particular chromosomes (X, Y, II, III, IV), by methods, by chemical substances studied, and by mutants; a geographical index; and a systematic index for the Drosophilidae and *Drosophila* species. The work is evidently a labor of love, and as fine a scientific bibliographical contribution as I personally know of.

BENTLEY GLASS

HEREDITY AND EVOLUTION IN HUMAN POPULATIONS. *By L. C. Dunn. Harvard University Press, Cambridge.* $3.50. x + 157 pp.; ill. 1959.

Readers who are well acquainted with *Heredity, Race, and Society*, by L. C. Dunn and Th. Dobzhansky, a book which in its paperback edition has reached many thousands of persons, will know in advance what to expect in the present volume: first of all, perfect lucidity in handling the subject; secondly, a comprehensive, balanced view of all its main aspects; and thirdly, evidence of a social conscience well-disciplined by respect for scientific evidence. The present book begins with a consideration of human variety and the principles of heredity applied to populations. A succeeding chapter then outlines the methods of evolutionary change in human populations, and considers their relative importance in varying circumstances. The significance of the presence in certain populations of extraordinarily high frequencies of the genes that produce sickle cell anemia and thallassemia, each a disabling and often fatal blood defect, is used to illustrate the matter. A chapter on Genes and Evolution is based largely on analyses of the human blood group systems, ABO and Rh in particular, and presents the recent evidence of an association between the ABO genotypes and certain other diseases and of the ABO interaction with Rh maternal-fetal incompatibility and sensitization. The preservation of rarer deleterious genes in the population, presumably mainly or exclusively by means of mutation, is also considered.

The three final chapters are entitled Race Formation, Isolated Populations and Small Communities, and A Look Ahead. The possibility of genetic drift occurring in small aboriginal human groups is

supported by genetic analyses of present-day religious isolates, such as the Jewish community of Rome studied by Dunn himself, the Pennsylvania Dunkers, and tribes of Australian aborigines. Differentiation in larger groups, such as the Hindu castes and the Negro communities of the New World, is a phenomenon that helps to bring out the influence of variation in the size of population on the course of human evolution. Lastly, Dunn discusses the possibilities of a eugenic future for mankind, with proper caution and appreciation of the difficulties. As he says, with each advance in human power and each increase in control, with each increase in efficiency or effectiveness through the application of scientific method, there is a proportionate increase in the precariousness of human existence, and this must in turn require further scientific advance and control, "... a condition which ignorance, unreason, prejudice, even lack of rapid scientific advance, could quickly make entirely untenable for man. Once we have got the jump on nature, we shall have to keep on jumping faster and farther." It is wisdom as well as knowledge that will be required, especially the wisdom of humility which consists in recognizing that more of the task lies ahead than behind us.

Every literate citizen, and not just every college student in search of a liberal education, should read these tantalizingly brief chapters. They will provide an indispensable orientation in the alarms of debate over the future of our genes. Geneticists, too, will find the volume not only a model of exposition worthy of emulation, but a source of many fruitful thoughts.

BENTLEY GLASS

GENETIC RESISTANCE TO DISEASE IN DOMESTIC ANIMALS.

By Frederick Bruce Hutt. Cornell University Press including Comstock Publishing Associates, Ithaca. $3.50. xiv + 198 pp.; ill. 1958.

This slender volume grew out of a lifelong interest in genetics and its application to animal breeding, a series of lectures at North Carolina State College, and lectures to veterinary students at Cornell University. The author accurately states the scope and intent of the book in his Preface.

The numerous examples given are from many parts of the world and include references to plants and to animals ranging from invertebrates to man. The environment and its role in disease and mortality are fully recognized. The phrase "domesticated animals" is liberally interpreted in include oysters, bees, mice, and many others. The coverage is of necessity uneven, since much is known about some species, little or nothing about others. Cattle and chickens receive major emphasis. There is less about sheep, swine, rabbits, and turkeys, for example. The literature cited is minimal.

Hutt has written an excellent book for the veterinary student who will acquire some aspects of the subject from other sources. Because of its wide coverage the book should also be useful to the much wider group of those interested in different phases of genetic variation in the ability to survive.

V. S. ASMUNDSON

INTERNATIONAL REVIEW OF CYTOLOGY. *Volume VII. Edited by G. H. Bourne and J. F. Danielli. Academic Press, New York.* $16.00. x + 684 pp. + 6 pl.; text ill. 1958.

Two of the articles in the current volume are historical in approach; the remainder deal essentially with up-to-date appraisals of problems of more limited scope.

F. G. Spear presents a historical review of experimental radiology, and is more concerned with the establishment of precedents than with a comprehensive review of the subject. Many of the more modern aspects of the subject are omitted or but briefly mentioned, but the article serves a useful purpose when combined with other, up-to-date reviews. F. Haguenan's review is concerned with the history, ultrastructure, and biochemistry of the ergastoplasm (he discards the cognomen "endoplasmic reticulum" except as a morphological term). The review is an able piece of writing.

Special articles are as follows: The effect of carcinogens, hormones, and vitamins on organ cultures (I. Lasnitzki); Recent advances in the study of the kinetochore (A. Lima-de-Faria); Autoradiographic studies with S^{35}-sulfate (D. D. Dziewiatowski); Structure of the mammalian spermatozoon (D. Fawcett); The lymphocyte (O. A. Trowell); Structure and innervation of lamellibranch muscle (J. Bowden); Hypothalamo-neurohypophysial neurosecretion (J. C. Sloper); Cell contact (P. Weiss); Anatomy of kidney tubules (J. Rhodin); Structure and innervation of the inner ear sensory epithelia (H. Engström and J. Wersäll); and Isolation of living cells from animal tissues (L. M. Rinaldini).

Of these, the papers by Fawcett, Trowell, and Weiss are particularly interesting to the general biologist, that by Weiss being most provocative of ideas. Rinaldini's paper will be of interest to tissue culturists. The remainder are of more specialized and limited scope. All, however, are in keeping with the high standards set by the editors, such as to make this series a significant contribution to a science that impinges on many fields.

C. P. SWANSON

LOOKING AT CHROMOSOMES.

By John McLeish and Brian Snoad. St. Martin's Press, New York. $3.75. viii + 87 pp. + 48 pl.; text ill. 1958.

No other book of which I am aware handles mitosis and meiosis in so skilful a manner, or illustrates these two types of cell division so beautifully. It can be stated unequivocally that no better set of illustrations exists in the biological literature; the student who cannot understand the mechanism and meaning of cell division after reading this text, or be stimulated by the beauty of the illustrations, is unworthy of further instruction.

The material covered includes root-tip mitosis, microsporogenesis, and megasporogenesis in the Regal lily, with some additional information about reproduction and heredity. Line drawings complement the photomicrographs, and help to tie text and illustrations together into a sound, well-written presentation. The authors should be highly commended.

C. P. SWANSON

GENERAL AND SYSTEMATIC BOTANY

FIFTY YEARS OF BOTANY. *Golden Jubilee Volume of the Botanical Society of America.*

Edited by William Campbell Steere. McGraw-Hill Book Company, New York, Toronto, and London. $10.00. xiv + 638 pp.; ill. 1958.

In 1906 the Botanical Society, which had been in existence since 1893, merged with the morphologists, physiologists, and mycologists to found the present Botanical Society of America. The year 1956 was therefore a Golden Wedding anniversary rather than a birthday, as Tippo points out in the introductory article of this volume, and the Society celebrated it in two ways: it issued certificates of merit to fifty botanists who had made outstanding contributions to botanical science; and it designated Volume 43 of the *American Journal of Botany* (1956) as a Golden Jubilee Volume. Volume 43 (and Volume 44 as well) differ from preceding volumes by the inclusion of a series of invited articles dealing with the many facets of botany. These articles have now been incorporated into this book.

The 40 contributions reflect, of course, the thoughts, approaches, and interests of each author; consequently there are gaps and a lack of uniformity of writing and presentation. In the main, however, the total effect is excellent, for the volume provides a sweeping survey, a history of past botanical studies and an indication of future directions. It also provides, for those who like to associate personalities with achievements, good photographs of the botanists who were honored by the society with certificates of merit, and who have been the leaders of botany in a generation now passing.

One gains the feeling from reading this volume that botany is very much alive and pregnant with stimulating possibilities, yet curiously enough, botany is at present attracting to itself very few of our intellectually able young people. Within our universities and colleges, except where it is associated with agriculture, botany is a minority science both in a quantitative and qualitative sense. Is this merely a passing change, or does it represent a shift of a deeper and more permanent kind? Only the present and future generations of botanists can determine this. The present volume is at least a measure of what our goals might well be.

C. P. SWANSON

NOMENCLATURE OF PLANTS. *A Text for the Application by the Case Method of the International Code of Botanical Nomenclature. Chron. bot., No. 31.*

By Harold St. John. The Ronald Press Company, New York. $2.50 (paper). x + 157 pp. 1958.

Taxonomic botany has been regarded as basic for all approaches to this science in Europe since the time of Linnaeus, as is clearly demonstrated by the endless number of good floras and textbooks on the subject in almost every language east of the Atlantic. Biology in general and taxonomy in particular are taught there in the schools at early stages, so that the layman's knowledge of the plants in the field will often astonish the visitor from other lands. In the western hemisphere, however, this is different, although some small increase in the recognition of taxonomy seems to be reflected in the number of textbooks on the subject published in America in recent years. This may, however, be a fallacious judgment, as is perhaps indicated by the absence of taxonomy in many university curricula, and the dominance of so-called "biology"—really biochemistry—in departments which ought to be founded on studies of evolution and classification.

There are many phases of taxonomy, and all are not equally interesting for the beginner, and some are difficult to teach. One of these phases is nomenclature, or the art of naming and describing a plant. There are certainly valid reasons for the fact that most recent textbooks in taxonomy avoid giving detailed information on this subject, which usually is thought to be either drier than the herbarium specimens it requires as vouchers, or dustier than the old books needed for its proper execution. It is not easy to introduce students to all details of these procedures, but they ought to realize that even this subject can become very fascinating and yield

a satisfaction hardly shared by some similar approaches to botanical fields.

Very few authoritative and commendable textbooks of nomenclatural practices are known to me. The very best, in German, was published before the last two Botanical Congresses altered the International Code of Botanical Nomenclature. It is *Die Technik der wissenschaftlichen Pflanzenbenennung*, by Rudolf Mansfeld. It gives the history botanical nomenclature and reviews the rules in a very simple and clear manner. No comparable book in English exists, while Josef Dostál has recently published a similar but different treatise in Czech.

The *Nomenclature of Plants*, which approaches the subject from a new angle, was written by one of those few American botanists who, in the course of a long and brilliant career as a research taxonomist, explorer, professor, administrator, and prolific writer, has had unrivalled opportunities to assess the far too common lack of appreciation of the tactics and strategy of scientific taxonomy. The book—which the author bases on his own long experience in teaching the subject—may, however, be of less interest than expected, and it is unlikely to find its way to students or to increase their interest in the subject to any positive degree. Even the name is misleading. The book has very little to say about nomenclature. Instead of basing the treatment on the most interesting phases of nomenclature, like Mansfeld and Dostál, St. John prefers to approach the subject from the point of view which he calls the "case method," in which he proposes a long number of cases in taxonomy to be solved by the aid of literature available to the student. It is possible that in the hands of an able instructor with good facilities this method, and consequently the book, would prove an inexhaustible well of joy and information; however, only a few instructors are stimulating taxonomists with ideas of their own, and most universities lack the pertinent literature. Without these rare facilities, the case method is likely to kill any taxonomic interest very effectively.

The first six pages of the book form a kind of introduction. It contains statements which perhaps indicate the author's limited understanding of botanical fields outside taxonomy and nomenclature. It is curious to see that although he defines only three bonanical disciplines (naming them terms), namely, systematic botany, taxonomy, and nomenclature, he feels forced to announce his ignorance or conservatism by pointing out that "the new terms *neosystematics* and *biosystematy* are not used here, as they seem based on an overemphasis of the fact that in recent times genetics and ecology have provided some new evidence to be included with the morphologic in reaching taxonomic judgements." This has nothing to do with nomenclature, and such trite remarks are regrettable and show only a lack of understanding about those taxonomical approaches which in Europe and Asia have most strongly revived systematic botany in recent decades. Again, comments on such matters as the problem of the generic classification of the quince, pear, and apple are faulty, since they indicate a lack of insight into evolutionary classification and have, in fact, very little to do with nomenclature. Otherwise the introduction is readable and interesting.

The main part of the book comprises numbered lists of 958 taxonomical cases, or names of taxa, with references to their publication, and divided into 7 chapters. Since there is little or no information as to the division of these cases, or their selection and arrangement, even persons with some knowledge in nomenclatural taxonomy will have difficulty in grasping this, and the uninitiated will find it quite unintelligible. Personally, I found the list very interesting, though incomplete, and sometimes, probably unintentionally, biased. Would it not have been more effective if it had been made considerably shorter, and in part replaced by clear and representative cases of different kinds, solved in some detail as examples? As it is, incomplete examples are numerous from the first two pages of Chapter 2, as, for instance, when *Lastraea* is forgotten in case 8, even though a critical perusal reveals considerably more cases both thorough and very informative.

The book is doubtless a good reference work for those who want to select cases of nomenclature to show students how to work in this field. A good taxonomist could, however, find representative examples without such assistance, and nowhere will there be any need for so long a list of cases for the purpose of teaching rising taxonomists. In addition, should such a book be given to students who have not already received sufficient grounding in taxonomy for their interest in the field to be very well established, probably no better method could be used to prevent them from becoming taxonomists. One wonders if it would not have been wiser for the author not to publish this book at all and for the publisher to use his paper to present some more living and more progressive approach to this field, which has come into such great distrepute in America. Why not, for instance, an equally short introduction to biosystematics, which the author of the book under review seems to deplore as a modern approach of little interest? That approach is based on evolution, and studies of evolution elsewhere have proven to be very attractive to young minds, though in their later years they may perceive the importance and fascination of nomenclatural taxonomy.

Áskell Löve

THE ORCHIDS. *A Scientific Survey. Chron. bot., No. 32.*

Edited by Carl L. Withner. The Ronald Press Company, New York. $14.00. x + 648 pp. + 61 pl.; text ill. 1959.

NATURAL HISTORY OF THE PHLOX FAMILY. *International Scholars Forum. Systematic Botany. Volume I.*

By Verne Grant. Martinus Nijhoff, The Hague. Guilders 19.— xvi + 280 pp. + 1 pl.; text ill. 1959.

Our most penetrating insight into the biology of the plant world has come from studies made of a single species or on a group of related species. Maize, *Datura*, *Crepis*, *Nicotiana*, *Tradescantia*, the tarweeds, and *Trillium*, to mention the more obvious ones, have yielded information which has greatly enriched our knowledge of the interrelationships of species in an evolutionary sense, and of their behavior genetically, cytologically, and ecologically. The present two volumes continue in this tradition, with Grant's volume hewing close to the line already laid down, while that on the orchids is oriented more to satisfy the needs of the orchidophile than those of the more scientifically trained botanist.

The orchid volume is an attempt, by 17 different authors, to assemble all that is known of the biology of these plants. It covers their classification, anatomy, variation, embryology, cytology, genetics, physiology, and pathology, with miscellaneous chapters on the mycorrhiza of orchids, photoperiodic responses, vanilla, orchid culture, and the making of chromosome counts. The orchidophile who is not well-trained in biology is likely to find the reading rather difficult, for little attempt has been made to bring the material down to a popular level; for the serious student, however, there is provided a great deal of solid information.

Grant's book is the first of a two-volume project. In his words, his goal is "to treat the Phlox family . . . as a model of evolution in a group of higher plants. The family is to be viewed as a model, not in the abstract sense of the theoretical and mathematical evolutionist, but rather in the sense of the naturalist—as a complex and far-flung production of nature." The present volume begins with a taxonomic survey, and is followed by chapters dealing with chromosome numbers, karyotype evolution, phylogeny, and phytogeography. Some 132 species, or 41 per cent of the family, have been examined cytologically. Nine is the original basic chromosome number, but reductions have taken place in advanced species to give 8, 7, and 6 as new basic sets. The evolutionary pattern follows generally that already determined within the genus *Crepis*. Tetraploidy is fairly common, but octoploidy has been discovered only once. In a phytogeographic sense, the Phlox group has appeared to follow the developmental route of many American species, arising out of the American tropics and advancing northward into western North America and on into boreal regions.

No new concepts or conclusions have come out of Grant's work, but his studies reinforce the view that one should look at the total aspect of a group if it is to be understood in a dynamic sense. The book is, therefore, a sound contribution to our botanical literature, to be further bolstered when the second volume, dealing largely with experimental findings, is forthcoming.

C. P. SWANSON

FLORA OF GUATEMALA. *Fieldiana: Bot., Vol. 24, Pt. 1.*

By Paul C. Standley and Julian A. Steyermark. Chicago Natural History Museum, Chicago. $8.00 (paper). x + 478 pp.; ill. 1958.

The plant world of Central and South America is less known than that of other continents, and it is being studied mainly by foreign visitors and collectors, notably from North America, but also from other countries as remote as Sweden. Many collections have been made, and a considerable number of papers have been written about the floras of these regions, although much is still unknown about their vegetation. A number of scientific accounts under the title of "Floras" have been published in the past for certain parts of the continent, and especially botanists connected with the Chicago Natural History Museum have worked out treatments in recent decades. One of these is the *Flora of Guatemala*, which in fact would not be incorrectly described as a treatment of the plants of tropical Central America. The present volume includes some families of gymnosperms and monocotyledons, although the largest families of these plants have been treated in previous volumes.

Each family and genus included in the present part is described concisely, with a reference to some recent monographic paper, and the description is then followed by concise keys to the respective genera or species. Every species is named with its Latin and native name, followed by taxonomical references and a description and notes on its distribution and uses, if any. Very many of the treatments of the species are, in addition, accompanied by exact and informatory drawings of necessary details, which undoubtedly facilitate their identification.

The Flora of Guatemala in general is an undertaking of great importance for the future, and it is to be hoped that the authors will be able to add very much more to the botanical knowledge of

this land which has so fascinated them, as they demonstrate in their Introduction. To write this *Flora* is an undertaking of great optimism, and those who do such painstaking work in such a different region from their homelands are worthy of every encouragement from their more sophisticated colleagues who prefer to work in areas wherein all such pioneer work has long since terminated.

ÁSKELL LÖVE

HOW TO KNOW WESTERN TREES.

By Harry J. Baerg. William C. Brown Company, Dubuque. $2.50 (cloth); $2.00 (paper). vi + 170 pp.; ill. 1955.

Science can be popularized in many different ways and most of the books that avoid too erudite a language will probably claim the greatest success. Also, while the facts of biology may be taught by different approaches, those that are most successful have based their explanations on plants or animals which people already know and want to understand more fully.

The publishing firm of Wm. C. Brown Company in Dubuque, Iowa, seems to have followed these leads when planning its Pictured-Key Nature Series, which was initiated in 1943 and has already resulted in 16 concise and inexpensive booklets. Some of these, naturally, are so specialized that the layman does not easily appreciate them, while others are made for the man in the street and especially for urban people who long to understand life in their own backyards and enjoy the living world outside the cities whenever they have an opportunity to pay it a visit.

The latest book in this series is a flora of the trees of the Rocky Mountains and the areas to the west. Not only is it a very simple and excellent descriptive key to these trees, with simple and effective drawings, but the book is also a textbook which in a few sentences teaches a non-botanist many of the facts he ought to know. A journalist or an artist looking through this book will never again believe in the upward stretching of trees, the home-gardener will be more careful not to hurt the cambium on the tree-trunks in his backyard, and will also be more careful in planting trees around his home, so that they will give him pleasure rather than trouble in later years. Those who want to experiment with trees can learn how to graft them in different ways, and simple pruning and strengthening of older trees is described lucidly. Even the methods of measuring trees are explained along with many hobbies, studies, and projects to be initiated with trees in garden or field.

The keys themselves fill the largest part of the book. Whenever the author cannot use common words and must resort to the terminology of the botanist, these terms are thoroughly explained. The most characteristic features of all the trees and shrubs included in the book are given in small but good drawings, and a small map indicates the distribution of the tree in western North America.

This little book can be warmly recommended, not only to those westerners who like to wander around in the forests, but also to others interested in trees in general. And if an eastern botanist carries this booklet with him on his travels in the western parts of the continent, his pleasures will manifoldly increase.

ÁSKELL LÖVE

ECONOMIC BOTANY

AMERICAN AGRICULTURE. *Geography, Resources, Conservation.*

By Edward Higbee. John Wiley & Sons, New York; Chapman & Hall, London. $7.95. x + 399 pp.; ill. 1958.

American Agriculture, based on the author's extensive field observations and a review of the literature, presents a comprehensive survey of land utilization in the United States and of the environmental features—land, water, soil, climate—of the various agricultural regions. The underlying reasons for regional differences are pointed out, and the patterns and favorable and unfavorable resources of each region are discussed. Special emphasis is laid on the location, uses, and methods of conservation of the soil and water resources. A unique feature is the presentation of case histories of individual farms which illustrate good management and the intelligent use and conservation of the available resources.

The 3 chapters of Part I furnish general background information regarding land resources in general and their use; the effect of climate, particularly moisture and temperature, on agriculture; and the nature, classification, and utilization of soils.

Part II is devoted to the dry West. Following a general account, specific subjects are taken up, such as forests and their multiple uses; the proper methods of irrigation; the growth, decline, and renovation of oases; and the use and management of ranges. The Palouse, Willamette Valley, and the dry plains are considered in more detail.

Part III is concerned with the humid East. The chapters include: The Humid East [general features]; The Origin and Crop Systems of the Corn Belt; Soil Conservation in the Corn Belt; The Economic Geography of Dairy Farming; General Farming in the Eastern Uplands; Land Use in the East-Central Appalachians; The Cotton Belt; Some Land

Systems in the Humid South; Specialty-Crop Areas in the Humid East (citrus fruits, rice, peanuts); and Some Specialty-Crop Areas (tobacco, market vegetables, potatoes, fruit).

The book is copiously illustrated with maps, charts, and field plans. A list of references follows each chapter and there is an index.

ALBERT F. HILL

THE ETHNOBOTANY OF THE ISLAND CARIBS OF DOMINICA. *Vol. 12, No. 2.*

By W. H. Hodge and Douglas Taylor. Instituto Botanico dell Universita, Firenze. (Paper). Pp. 513–644 + 40 pl. 1957.

This is a representative account of the ethnology of the Island of Dominica, and the use there of plants, wild or cultivated, for food or drink, for domestic purposes, and as medicines or magic drugs, followed by a thorough catalogue of more than 200 such species. It is interesting reading for specialists, and the well-made pictures are a joy to the eye.

ÅSKELL LÖVE

INTRODUCTORY HORTICULTURE.

By E. P. Christopher. McGraw-Hill Book Company, New York, Toronto, and London. $7.50. viii + 482 pp.; ill. 1958.

Many textbooks of horticulture are written primarily for the student and are only of secondary interest to others. *Introductory Horticulture*, however, although designed for a beginning course in the subject, is so well organized and has such complete coverage that it constitutes a valuable source of information for any gardener who wishes an understanding of the principles which are the basis of the various horticultural practices.

The first 6 chapters are general in nature and are entitled: Field of Horticulture; Classification, Structure and Growth of Plants; Climate and Horticulture; Soil and Soil Amendments; Plant Propagation; and Plant Growing Structures.

Seven chapters deal with specific fruits, vegetables, and flowers. In each case the general conditions affecting the growth of the plant are given, followed by a discussion of actual horticultural practices. Two chapters are devoted to vegetable crops and one each to outdoor floriculture, indoor floriculture, tree fruits (pomes), drupe and evergreen fruits, and small fruits.

The last 5 chapters are concerned with matters of more general interest and include: Home Landscape, Nurseries and Arboriculture, Plant Growing Problems: Abnormalities and Pest Control, Storage and Marketing, and Horticultural Shows and Judging.

A list of questions and a few selected references are appended to each chapter. There is also an adequate Index.

ALBERT F. HILL

THE GUIDE TO GARDEN FLOWERS. *Their Identity and Culture.*

By Norman Taylor; illustrated by Eduardo Salgado. Houghton Mifflin Company, Boston. $4.95. xx + 315 pp; ill. 1958.

There are several excellent garden encyclopedias, but often the reader is overpowered by the wealth of material contained in them. Consequently, the publication of a compact volume, limited to the identification and culture of the species anyone can grow in his own garden, is most welcome.

In *The Guide to Garden Flowers*, the author, widely known for his *Encyclopedia of Gardening* and other horticultural books, discusses 412 species in simple language and with a minimum of technical terms. These species include not only all the garden favorites but some less familiar varieties as well. Identification of the flowers is made easy by a carefully prepared key and by the illustrations—324 in full and remarkably accurate color and 88 in black and white.

The text contains information regarding the color, height, texture, design, habit of growth, flowering dates, varieties, soil requirements, culture, special care, and country of origin of the plants. For the most part the species are discussed by families. There are, however, special sections devoted to vines, bulbs, and everlastings. Both common and Latin names are consistently supplied. In addition there are reference lists of species arranged by flower color, preferred habitat, height, and season of bloom. Other sections list especially fragrant flowers, perennial ground covers, plants which need especial care, and annuals.

A good index and a short list of reference works are included.

ALBERT F. HILL

COMMERCIAL FLOWER FORCING. *The Fundamentals and Their Practical Application to the Culture of Greenhouse Crops. Sixth Edition.*

By Alex Laurie, D. C. Kiplinger, and Kennard S. Nelson. McGraw-Hill Book Company, New York, Toronto, and London. $9.50. vii + 509 pp.; ill. 1958.

Commercial Flower Forcing, useful either as a textbook for students of floriculture or as a guide for commercial florists, deals with all phases of commercial flower growing under glass. Among the subjects considered are: the status of the industry; selection of the site; erection of the physical plant; applied

plant physiology; cultural directions for cut flowers and potted plants; marketing; and costs of production.

This sixth edition takes into account the many refinements in the culture of plants which have taken place since 1948 (5th ed., *Q.R.B.*, 23: 354. 1948). Some techniques have been discarded. The production picture has been altered as a result of new studies of photoperiodism and temperature, and by changes in transportation facilities and in marketing practices. Two new subjects have been introduced: greenhouse air-conditioning and plastic greenhouses.

The Table of Contents includes: Status and Development of the Industry; Greenhouses and Related Structures; Greenhouse Heating and Cooling; Environmental Factors Influencing Plant Growth; Soils; Gravel Culture; Fertilizers; Reproduction; Diagnosing Plant Disorders; Major Cut Flower Crops (carnations, chrysanthemums, orchids, roses, snapdragons); Miscellaneous Cut Flower Crops; Bulbs, Corms, and Tubers; Flowering Pot Plants; Foliage Plants, Cacti, and Succulents; Bedding Plants; Marketing; and Costs of Production. There is a good Index.

Some of the material in this book, especially the chapters devoted to Bulbs, Corms, and Tubers, Flowering Pot Plants, and Foliage Plants, Cacti and Succulents, and the section on Keeping Qualities of Cut Flowers, will interest the layman who grows plants or has flowers in his own home.

ALBERT F. HILL

AN OUTLINE OF BRITISH CROP HUSBANDRY. *Third Edition.*

By H. G. Sanders. Cambridge University Press, London and New York. $6.50. x + 346 pp. + 2 pl. + 1 folded chart. 1958.

This third edition of a well-known textbook has been thoroughly revised and brought up to date. Data concerning new materials and methods have been included, especially in regard to the increasing use of compound fertilizers. As in the previous editions, the emphasis is on broad principles rather than details. Information regarding specific varieties, yields, and uses of common crops and grassland is either omitted or the references are only incidental. Consequently, the book presents for the most part an over-all picture of the science and practice of agriculture in Great Britain which will be of interest and value to American agriculturists, as well as to students and practical farmers in Britain itself.

The topics discussed include: rotations; manuring; cleaning; tillage and the requirements for a seedbed; the preparation of the seedbed; the choice and treatment of seed; sowing the seed; after-cultivation; the corn harvest [cereals and legumes]; harvesting by combine; the harvesting of roots and potatoes; and costs. There is an adequate Index.

ALBERT F. HILL

DISEASES OF TOBACCO.

By George B. Lucas. The Scarecrow Press, New York. $10.00. 498 pp.; ill. 1958.

Tobacco has long been an important crop in the United States. In recent years its production has been greatly limited by the increasing prevalence and severity of various diseases. This book has assembled the basic information on the nature and control of tobacco diseases. All pertinent papers, especially those published since 1935, have been reviewed. Diseases of economic importance in other countries are included, as well as those of the United States. Special attention has been paid to nematode diseases, disease-resistant varieties, cropping sequences in relation to disease control, the interaction of plant pathogens in relation to disease severity, and the impact of variability of the plant pathogens on disease control.

Part I presents general information under the following chapter headings: Tobacco—The Sovereign Weed; Plant Beds in Relation to Disease Control; Rotations in Relation to Tobacco Diseases; and Breeding Tobacco for Disease Resistance.

The remaining 7 parts are devoted to individual diseases: (II) Nematode Diseases; (III) Fungus Diseases; (IV) Bacterial Diseases; (V) Virus Diseases; (VI) Diseases caused by Flowering Plants; (VII) Malnutritional Diseases; (VIII) Miscellaneous Diseases.

The book, which has been printed by offset, is well illustrated. Extensive references are appended to each chapter and there is an unusually complete index.

ALBERT F. HILL

GENERAL AND SYSTEMATIC ZOOLOGY

GENERAL ZOOLOGY.

By Gairdner Moment; edited by Bentley Glass; illustrated by Elmer W. Smith. Houghton Mifflin Company, Boston. $7.50. xii + 632 pp.; ill. 1958.

Moment's textbook represents something of a departure from the conventional approach to introducing students to the field of zoology. General biological principles are stressed rather than a systematic and extensive consideration of the animal kingdom. The style should present interesting reading to the beginning student, and the book is filled with excellent illustrations. This textbook is ad-

mirably suited to a course given as part of a general education in a liberal arts college, particularly in programs where such a course would represent the student's only contact with zoology during his college career. It might also be useful in a department where advanced courses in invertebrate zoology and vertebrate anatomy constitute a usual part of the undergraduate curriculum, thus insuring these students of becoming adequately exposed to classical zoology. However, this textbook would not appeal to those instructors who feel that the beginning course should stress classical zoology.

RONALD R. COWDEN

INSECTS AND MITES OF WESTERN NORTH AMERICA. *A Manual and Textbook for Students in Colleges and Universities and a Handbook for County, State and Federal Entomolgists and Agriculturists as Well as for Foresters, Farmers, Gardeners, Travelers, and Students of Nature.*

By E. O. Essig. The Macmillan Company, New York. $18.00. xiv + 1050 pp.; ill. 1958.

It is most unfortunate that this standard reference text has been republished under the guise of a revised edition that provides "... an up-to-date source for correct spelling of the common and scientific names of insects." Even more culpable is the assertion that the vast number of references "... make Essig a definitive authority." These statements were true—33 years ago when the first edition of this book appeared. American entomology is deeply indebted to the author and publishers of that edition and for its subsequent printings of 1929, 1934, and 1938. However, publication of the present "revision" constitutes a serious disservice to the continued development of the science.

In his Preface to the 1958 edition, the author properly notes the phenomenal developments in insect control since 1944, and comments on the great strides of systematic entomology made since 1926. Unfortunately, this edition gives little evidence of these developments. The most significant changes concern recommendations for control and the substitution of new figures for outmoded control equipment. The new figures usually show economically important species of insects or mites and do add somewhat to the usefulness of the book. On p. 44, an obsolete key to genera of Eriophidae (mites) was replaced by illustrations of the citrus bud mite and designated Fig. 27 without removing or renumbering the old Fig. 27 on the following page. Recommendations for grasshopper control (p. 73–74) are updated to 1952. Otherwise there are few changes of consequence in the first 100 pages.

Of 258 references contained in the first 100 pages, 190 predate 1916 and only 4 refer to papers published since 1925. Yet this section includes the mites, which have come into prominence during this period as increasingly serious pests. Neither is there adequate bibliographic notice of the revolutionary taxonomic changes that have affected this group during the past 15 years.

With few exceptions, the scientific names used throughout the book are those which were in vogue circa 1925. Names such as *Halticus citri* (*Halticus bracteatus*), *Triphleps tristicolor* (*Orius tristicolor*), and *Thyreocoris extenus* (*Corimelaena extensus*) have been obsolete since 1926, the year of the first publication of this book. In the suborder Heteroptera, the group with which I am most familiar, 156 species or subspecies are referred to by name. Of these, 17 are incorrect because of synonymy affecting either the generic or trivial name and a total of 26 names refer to species that have been affected in some way by changes in the zoological concepts of either the genus or species. In the suborder Homoptera the situation is worse, for there has been a correspondingly greater change in the systematics of this group. Of the 35 species of leafhoppers treated, 14 are referred to by obsolete names. Two additional names are misspelled.

The economically important beet leafhopper was known in 1925 as *Eutettix tenellus*; in the present edition, it is called *Norvellina tenellus* on p. 211, but still referred to on pp. 350 and 357 under the earlier name. Actually the species has been known as *Circulifer tenellus* since 1948. Even where corrections have been made, there is a disturbing absence of consistency. On p. 213, the name of the mountain leafhopper was corrected to *Collodanus montanus*, but the preceding and following species are still called *Thamnotettix*, though the former also belongs to *Colladanus* and the latter to *Graminella*, a very different genus.

If it had been necessary to search a scattered literature to find the correct names for leafhoppers, such discrepancies would be less surprising. However, a *Generic Classification and Check List for the Nearctic Leafhoppers*, by P. W. Oman, was published in 1949. Unquestionably Essig knew of this publication, for he contributed to its Preface and in his book it is included among the references listed (p. 206). Also surprising is the complete omission of any reference to the economically important spotted alfalfa aphid (*Therioaphis maculata*). Although this insect was not discovered in California until 1955, its importance in the southwestern United States since that date has been such that it could not have escaped the author's attention.

A casual survey of other insect orders shows no evidence of any important changes of the kind needed to make this book an up-to-date treatment of the insects of Western North America. As a ref-

erence book, it retains much of the merit it has continued to enjoy through the years, but contrary to the impression one might acquire from a comparison of the 1926 and 1958 editions, entomology has not been a static science. No one owning a still serviceable copy of the first edition should invest in the "revised" version. His money can be used to better purpose.

R. I. Sailer

Trackways of Living and Fossil Salamanders. *Univ. Calif. Publ. Zool., Vol. 63, No. 1.*

By Frank E. Peabody. University of California Press, Berkeley and Los Angeles. $1.50 (paper). iv + 48 pp. + 11 pl.; text ill. 1959.

A Field Guide to Reptiles and Amphibians *of the United States and Canada East of the 100th Meridian. The Petersen Field Guide Series.*

By Roger Conant; illustrated by Isabelle Hunt Conant. Houghton Mifflin Company, Boston. $3.95. xviii + 366 pp. + 40 pl.; text ill. 1958.

This is an excellent book. It is similar in design to Peterson's familiar *Bird Guides* but is illustrated primarily with photographs rather than with drawings emphasizing gross features. This difference in approach was dictated by the fact that birds are usually observed at some distance, whereas most reptiles and amphibians would be in the hand or at close range, where the details of their structure could be studied.

As the first step in identification, Conant recommends that the animal be compared with the plates, which are grouped according to relationships. In each plate, or section of a plate, the pictures are to scale (although the scale is not indicated). Opposite the plates are the common names of the species, a mention of a couple of important distinguishing characters, and the page references for the text description and the range map. A recommended list of standardized common names for reptiles and amphibians has recently been prepared by a committee (of which Conant was chairman) of the American Society of Ichthyologists and Herpetologists, and these recommendations are followed in the *Guide Book*.[1] Nevertheless, it would have been desirable to give the scientific names with the plates, since, in many instances, the more sophisticated users of the book would find them more meaningful than the common names. Key characters are frequently emphasized on the plates by the use of an index line, as in the *Bird Guide*.

Conant spared no effort in making the book as accurate and as useful as possible. The text and range maps were reviewed by specialists working on particular genera or investigating specific herpetofaunas. The roster of acknowledgments is a list of virtually all of the active herpetologists in the United States. The job of photographing, in life, the species illustrated was itself a terrific chore, very ably handled by Mrs. Isabelle Conant. Over 3000 live specimens were employed and formed the basis for about 1100 illustrations, of which over a third are in color.

About half a page is devoted to a text account of each form, with cross references to the pages where it is illustrated and where its range is mapped. The descriptions concern aspects of identification primarily, but ecological and other matters are also discussed. A useful feature is a brief paragraph comparing the form with other species with which it might be confused. Text figures complement the descriptions of characteristics.

The 28-page Introduction is a valuable part of the book. It contains a good deal of useful information that will be of particular interest to amateurs. Conant writes well, and I am sure that persons wishing to learn more about reptiles and amphibians will find this book almost as easy to read as a good novel. There are an adequate index, a glossary, and a short bibliography.

The handling of the problem of species and subspecies was done intelligently. Rather than attempting to be "consistent" and to give full coverage to all species (or all subspecies), Conant has treated forms that were easily recognizable regardless of their current systematic position. All races in the "Check List," however, are listed even though not all are given full and separate coverage.

The last twenty years has witnessed the publication of a large number of very excellent guides (or handbooks) in the field of herpetology—some regionally oriented and some devoted to special groups. For the most part, they have been excellently done, and Conant's fine book joins distinguished company. Collectively, these handbooks furnish an indication that American herpetology has become a mature descriptive science. They are a monument to the thoughtful and intensive research carried on by a large number of professional and amateur naturalists.

Arnold B. Grobman

Variation in and Distribution of Lizards of Western Mexico Related to *Cnemodophorus sacki. Bull. Amer. Mus. nat. Hist., Vol. 117, Art. 2.*

By Richard G. Zwiefel. American Museum of Natural History, New York. $1.25 (paper). iv + pp. 61–116 + pl. 43–49; text ill. 1959.

THE BIRDS OF ALASKA.

By Ira N. Gabrielson and Frederick C. Lincoln; illustrations by Olaus J. Murie and Edwin R. Kalmbach. The Stackpole Company, Harrisburg; Wildlife Management Institute, Washington. $15.00. xiv + 922 pp. + 13 pl. 1959.

This volume follows the tradition of the state bird books and has the customary chapters on life histories, migration, and life zones. An innovation is a chapter on introduced game birds. For each species there are provided a detailed description of the bird, a statement of its range in general and in Alaska, and a discussion of its haunts and habits. It would have been valuable to comment on any special attributes that a species might have in this northern climate. The book is concluded by a gazetteer, bibliography, and index.

DAVID E. DAVIS

ECONOMIC ZOOLOGY

INVESTIGATIONS OF RING-NECKED PHEASANTS IN ILLINOIS. *Illinois Dept. Conserv. Div. Game Management. Technical Bull., No. 1.*

By William B. Robertson. Department of Conservation, Springfield. Free upon request (paper). iv + 137 pp.; ill. 1958.

The Illinois Department is to be congratulated on the appearance of its first Technical Bulletin. If succeeding volumes are as good as this one, the series will become an influential contributor to the science of wildlife management. Pheasants have been intensively studied in Illinois from 1946 through 1951, and the results of these investigations are reported here. The history of this introduced bird is traced from its beginning to its present status. Actually too much material is presented and too many detailed facts are given to enable a reasonable review to be attempted. A listing of the topics that are discussed demonstrates that nearly every aspect of field work has been undertaken: land use and its relation to pheasant studies; winter observations (including weight changes, age ratios, movements, behavior); spring populations of cocks; spring sex ratios; nest and brood studies; hunting season studies; releases; population trends; and factors that limit pheasant distribution. All of the conclusions reached in all of these topics are carefully compared with the results obtained by other workers in other areas. The reader can also form his own interpretations from the abundance of tables (61) and of figures (27) so generously supplied. This publication is necessary for anyone interested in game birds and their management.

HENRI C. SEIBERT

ANIMAL GROWTH AND DEVELOPMENT

EMBRYONIC NUTRITION. *A Report from the Developmental Biology Conference Series, 1956, held under the Auspices of the National Academy of Sciences—National Research Council.*

Edited by Dorothea Rudnick. The University of Chicago Press, Chicago. $3.25. xii + 113 pp.; ill. 1958.

ENDOCRINES IN DEVELOPMENT. *A Report from the Developmental Biology Conference Series, 1956.*

Edited by Ray L. Watterson. The University of Chicago Press, Chicago; The National Academy of Sciences—National Research Council. $4.00. xiv + 142 pp.; ill. 1959.

DYNAMICS OF PROLIFERATING TISSUES. *A Report from The Developmental Biology Conference Series, 1956.*

Edited by Dorothy Price. The University of Chicago Press, Chicago; The National Academy of Sciences—National Research Council. $3.25. xvi + 96 pp.; ill. 1959.

ENVIRONMENTAL INFLUENCES ON PRENATAL DEVELOPMENT. *A Report from the Developmental Biology Conference Series, 1956.*

Edited by Beatrice Mintz. The University of Chicago Press, Chicago; The National Academy of Sciences—National Research Council. $3.00. xiv + 87 pp.; ill. 1959.

These volumes represent four of the ten which report the proceedings of the Developmental Biology Conferences of 1956, organized under the general chairmanship of Paul Weiss. The volume entitled *Embryonic Nutrition*, which reports the communications and discussions of the symposium on that subject arranged by J. S. Nicholas, is the only one of the four which presents the full papers as delivered. These were given by E. J. Boell, J. Lee Kavanau, Sydney Smith, James D. Ebert, Nelson T. Spratt, Jr., and Florence Moog.

The report on the symposium on *Endocrines in Development* is a summary, made by the editor, of the conference discussions. It deals with the ontogeny of specific vertebrate endocrine glands (thyroid, adrenals, gonads, parathyroids) and of endocrine or hormone-dependent receptors in the vertebrates (gonads and gonoducts, feather papillae, and pigment cells).

One chapter devotes itself to the endocrine glands of arthropods, especially insects. In addition, several more general topics are considered, such as synergy and integration within the endocrine system, and the mode of action of hormones.

The symposium on the *Dynamics of Proliferating Tissues*, also summarized by the editor, treats of the

types of tissues which proliferate constantly, deals with methods for estimating cell loss and rates of proliferation, and takes up some of the factors which influence rates of cell loss and proliferation. The symposium on *Environmental Influences on Prenatal Development*, again as reported by the editor, considers the effects of teratogenic agents, including genetic factors, with regard to their specificity, and the actions of metabolic inhibitors and other extragenic factors, including those relating to the uterine environment of the mammal, on growth and development.

Each of the volumes has an identical Preface (the Preface to the series) by Paul Weiss, and each has a bibliography. The bibliographies of the four volumes noticed here all include references dated 1957; one has a single 1958 reference in addition, and two others have two. All of the volumes are illustrated, and none is indexed. There is considerable disparity in the fluency and skill with which the proceedings are described in the three volumes in which the editors have reported the discussions; but so also are there differences in the quality of the communications in the volume in which the authors have spoken for themselves. Personally, the volume which presents the authors' results and thoughts in their own words seems the most useful as a permanent record. But the discussions as reported in the other volumes are all both interesting and stimulating, and will prove of value to students who read them while attempting to develop their own perspectives.

JANE OPPENHEIMER

MITOGENESIS. *A Report from the Developmental Biology Conference Series, 1956.*

Edited by H. S. Ducoff and C. F. Ehret. The University of Chicago Press, Chicago; The National Academy of Sciences–National Research Council. $3.25. xiv + 114 pp.; ill. 1959.

This is an edited transcript of a conference held at the Argonne National Laboratory, in which "mitogenesis" has been considered in a broad sense to concern factors which control or initiate cell division. These, to mention only a few of the topics discussed, include premitotic growth, growth promoters and inhibitors, the mitotic spindle, differentiation and division, and the radiation sensitivity of division versus differentiation.

Doubtless these conferences were intended to stimulate new ideas and to synthesize information by bringing together prominent scientists working on various aspects of this problem. Unfortunately, there is very little originality, either of ideas or information, to be found in this transcript. A scientist interested in this specific area should, however, read these proceedings, on general principles. The late appearance of this whole series of books—at least two years after the meetings—has also detracted from their value, since neither the material nor the bibliography is really current.

RONALD R. COWDEN

CYTODIFFERENTIATION. *The Developmental Biology Conference Series, 1956, held under the Auspices of The National Academy of Sciences and National Research Council.*

Edited by Dorothea Rudnick. The University of Chicago Press, Chicago. $3.75. xii + 148 pp.; ill. 1958.

Probably no problem in biology has been such an enigma as that of cellular differentiation, and no biological problem is in greater need of solution. The publication of this book, one of ten in the 1956 Conference Series organized by Paul Weiss, makes these points clear, while at the same time presenting in able fashion our current views.

The question of terminology has been explored, and the overall picture appraised without any definitive conclusions being drawn. The discussions, however, were useful in sharpening points of view and in reconciling, at least in some instances, seemingly disparate positions. The discussions revolved around several foci: genic endowment; chemical and morphological indices; myogenesis and fibrogenesis as special cases of cytodifferentiation; the internal milieu; cancer; and growth. Despite the enigmatic nature of the problem, the discussions had an optimistic note, and it is clear that the book should prove a boon to future experimentation. The editor of this volume should be congratulated on the organization and clarity of her presentation.

C. P. SWANSON

WOUND HEALING AND TISSUE REPAIR. *The Developmental Biology Conference Series 1956 held under the auspices of The National Academy of Sciences—National Research Council.*

Edited by W. Bradford Patterson. The University of Chicago Press, Chicago. $2.75. xii + 83 pp.; ill. 1959.

This is another edited transcript of one of the 1956 Developmental Biology Conference Series, organized by Paul Weiss. The seven topics herein discussed are as follows: Biological aspects of wound healing; Cellular components of the wound; The ground substance; Wound healing and sulfur metabolism; Systemic factors acting on the healing wound; The healing of bone; and Abnormal healing. While this was not a completely general conference on wound healing—inasmuch as very little discussion of experimental work on wound healing in invertebrates

was included—it does offer in the first five topics a discussion of the histogenesis of vertebrate wound healing (both at the microscopic and submicroscopic levels), some histochemical studies on that sequence of events, both histological and biochemical studies of polysaccharides and collagen in wounds, some important aspects of sulfur metabolism in wounds, and the effects of endocrines on wound healing. This represents a fairly comprehensive coverage of the problems in this area which are being actively studied. In this respect, the publication of the transcript should be well worth while, both to those who would like to be brought up to date in this area and to anyone else interested in the general biological problems involved in wound healing. While the bibliography is not completely up to date, an attempt has been made to include at least some of the post-1956 references, so as to lessen the criticism that this book is appearing so long after the termination of the conference.

RONALD R. COWDEN

ANIMAL MORPHOLOGY

DISSECTION GUIDES. *I. The Frog. II. The Dogfish. III. The Rat, with Notes on the Mouse. IV. The Rabbit. V. Invertebrates.*

By H. G. Q. Rowett. Rinehart & Company, New York. 95 cents each (paper). (I) 63 pp.; ill.; (II) 62 pp.; ill.; (III) 64 pp.; ill.; (IV) 32 pp.; ill.; (V) 56 pp.; ill. 1957.

It came as a genuine surprise, to one who has seen more dissection manuals than he cares to remember, to find this series a delight to examine. There is a sharp and handsome clarity of drawing which reveals precisely where to cut, probe, and fold, and how to place the scalpel or forceps. Moreover, the drawings are numerous, fully labelled, and very informative. In fact, those of the rat and rabbit are masterpieces in presenting a great wealth of detail without a sense of confusion. Written instructions are held to a minimum and are given in crisp but adequate statements. Anatomy is presented as one of the exact sciences, as clean and as definitive as Euclid. It is safe to assume that students will enjoy and profit by these manuals.

An obvious fault is the quite inadequate treatment of the muscular system. Muscles are mentioned only incidentally in the dissection of other systems. For some purposes this may be sufficient; yet muscles are a major part of vertebrates, and those of the shark are unexcelled for showing in a very simple form the basic vertebrate pattern seen only with difficulty in the mammals. Where anatomy can be tied into physiology, this of course should be done in an elementary course; yet the student will look in vain here for a dissection of the frog's gastrocnemius and its innervation, or for the physiologically very different locking and closing portions of the clam adductor. The semicircular canals of the shark receive rather summary treatment, although these furnish one of the most elegant dissections in the entire field of anatomy, vertebrate or invertebrate.

There are suggested student drawings which, as Louis Agassiz discovered so long ago, can be used to force students to do accurate and detailed observation. However, most of these suggestions would be more challenging if the student were asked to draw the given structure from a point of view quite different from that already provided by the manual. This is a fault which the instructor may easily remedy.

The differences between these manuals and those of the earlier English edition are negligible or nonexistent. This is evidently all to the good, although it is a pity to find that *Octolasium* has been deleted from the table of earthworms available for dissection, for it is common enough in rich soil, at least in eastern North America. No use of latex injections is mentioned.

GARDNER B. MOMENT

ESSENTIALS OF HUMAN ANATOMY.

By Russell T. Woodburne. Oxford University Press, New York. $12.50. viii + 620 pp.; ill. 1957.

This new book is an expression of the prevailing style in anatomical textbooks which emphasizes double-column text, simplicity in illustrations, and concise brevity. The organization, after an introductory section on general concepts, is by regions of the body. The illustrations are mostly line drawings interspersed appropriately in the text. The text itself, which uses the "Paris Nomenclature" (1955), is accurate and clear. The 18-page Index seems rather short for an anatomy text but it appears to be adequate. This new anatomy book is written carefully and concisely along popular lines by an anatomist of established reputation. There is nothing radically new in its approach to a subject that has been written about for more than a century, but its high quality deserves a favorable reception.

F. N. Low

CUNNINGHAM'S MANUAL OF PRACTICAL ANATOMY. *Volume 1. General Introduction, Upper Limb, Lower Limb. Twelfth Edition.*

Revised by James Couper Brash. Oxford Univer-

sity Press, New York, Toronto, and London. $6.00. xii + 394 pp. + 44 pl.; text ill. 1957.

This well-known manual, now in its 12th edition, continues its established policy of including sufficient detail to insure thoroughness by the user. But the order of regional presentation is altered to accomodate individual differences in dissecting schedules, so as to lend greater flexibility to its use. The Birmingham Revision (1933) of anatomical nomenclature is used, with parenthetic mention of the same part in the "Paris Nomenclature" (1955) wherever an important difference occurs. The presentation as a whole is essentially unchanged from previous editions. There is no reason to anticipate that its status will undergo any change from its present one as a useful and popular dissecting manual of human anatomy.

F. N. Low

A Stereoscopic Atlas of Human Anatomy. Section IV, The Thorax. *Twenty reels ($113–132) of 7 stereoscopic views each in two separate booklets. By David L. Bassett; color photographs by Wm. B. Gruber. Sawyer's, Portland, Oregon.* $16.50. 1958.

"What to do that's new" is an arduous problem in the pedagogy of gross anatomy, and has been for so long that this excellent stereoscopic atlas is indeed a remarkable and refreshing solution. By combining modern refinements of the techniques of stereoscopy and color photography with the facilities of a first-rate anatomical laboratory, Bassett has turned out a pictorial representation of gross anatomy that ranks high in its field.

High quality color photographs of anatomical parts, dissections, etc., by W. B. Gruber are mounted in their final form, on a series of small (3½ inches) cardboard reels for use in any View-Master Stereoscopic Viewer. Each paper-bound booklet contains 10 reels under the front cover, the remainder of the volume being in the form of double-page spreads. Each spread corresponds to one of the stereoscopic views and consists of a numerically labelled line drawing on the right-hand page with a brief description and designation of these labelled parts on the left. The "Paris Nomenclature" (1955) is used, and the subject material portrayed reflects the careful and discriminating selection of a careful anatomist. This atlas should be indispensable to every anatomical laboratory and should be valuable wherever there is any interest in biology.

F. N. Low

Textbook of Comparative Histology. *By Warren Andrew. Oxford University Press, New York.* $15.00. xx + 652 pp. + 4 pl.; text ill. 1959.

This book represents an attempt to offer in an organized fashion some examples and information in the area of comparative histology which might form the basis of a course. Such an orientation for a histology course in a biology or zoology department would actually be more appropriate than the usual course, which is restricted mostly to mammalian organology. In this book the emphasis has been placed on the broader aspects of organ structure and function, and examples have been drawn from many animal groups. This is not, however, a comprehensive sourcebook of information on comparative histology, and because of this the examples presented naturally represent a selection. Since this selection is governed by a combination of factors, including the taste of the author and the availability of information about a particular group, there may be some who would take exception to the author's choices. The list of references at the end of each chapter is unfortunately too scanty to be of much value to a student with an exploring mind. Still, this is a unique book which offers a unique opportunity to histologists in biology and zoology departments to offer a broadening course in comparative animal histology rather than the usual dress rehearsal for a medical school course that covers the same ground.

Ronald R. Cowden

Human Histology. *A Textbook in Outline Form. By Leslie B. Arey. W. B. Saunders, Philadelphia and London.* $6.50 (paper). ix + 337 pp. 1957.

This outline of histology is presented with a threefold purpose: to use as a laboratory companion, to consolidate knowledge from other sources, and to use for review. A great deal of accurate factual information is presented in the form of short sentences or noun phrases. In addition to the introduction there are parts on cytology, general histology, and organology which are more or less orthodox in their organization. If the limitations of the outline approach to a subject, which invites hasty short-cuts, can be ignored, then this hard-hitting presentation is a commendable one.

F. N. Low

Liver: Structure and Function. *By Hans Popper and Fenton Schaffner. The Blakiston Division, McGraw-Hill Book Company, New York, Toronto, and London.* $20.00. xv + 777 pp.; ill. 1958.

This is a complete summary of present knowledge of the structure and function of the liver in health

and disease. It is based on a wide study of the literature interpreted through the experience of the authors in clinical practice, in the pathological laboratory, and in research. Directed primarily to the clinician, it is nevertheless a valuable source book for the anatomist, physiologist, and biochemist who deals with the liver.

The purposes and scope of the work are set forth in the Preface, from which the following statements are taken: "This book attempts to assemble current concepts of hepatic structure and function for the benefit of the physician. Emphasis is placed upon the basis, the method, and the practical application of each of the so-called 'liver function tests.' The diseases are classified on the basis of experience with liver biopsies. . . . only those aspects of hepatic physiology and pathology which have practical significance will be considered. . . . [this] is necessary to avoid an encyclopedic treatise on general physiology and biochemistry. . . . In general, clinical features which are obvious and well described in standard textbooks . . . are omitted. . . ." The limitations so imposed explain some lack of completeness—for example, in regard to the liver enzymes—whereas the hepatic function tests are treated exhaustively.

The book does not lend itself to a brief summary. Its 64 chapters and Appendix are organized into 8 parts, the titles of which give a summary of the contents and indicate the method of treatment of the subject matter. I, Normal Structure and Function; II, Pathologic Phenomena in the Hepatobiliary System; III, Clinical Study of Hepatic Function and Structure; IV, Diffuse Diseases of the Liver; V, Focal Diseases of the Liver; VI, Tumors of the Liver and Biliary Tree; VII, Internal and External Environment and the Liver; VIII, Appendix. The table of contents gives, in addition to the titles of the chapters, a topical analysis of each, making it a simple matter to locate the treatment of any particular subject. The Appendix is a practical summary of the Principles of Diagnosis of Liver Disease Based on Coordinated Use of Functional and Structural Observations. Being based on a thorough knowledge of the literature and on the wide experience of the authors, this book should be of great value to the clinician.

The illustrations are for the most part original, and were prepared expressly for this book or taken from publications of the authors. They are carefully selected, and the reproduction is excellent. Photographs of gross specimens and of microscopic preparations are both of very high quality. It is particularly fortunate that some of the fine illustrations from the *Ciba Collection of Medical Illustrations* could be used. There are a number of schematic diagrams to epitomize the complicated processes in which the liver is involved: e.g., Pathways of blood and bile pigment metabolism; Pressures and effects of ligations in the blood vessels of the liver; and Pathogenesis of ascites. These should be of great help to the student. A Bibliography of 3735 entries, many of them multiple, is comprehensive and complete. The continued references to it throughout the book, usually several in every paragraph, make the text in a sense an annotated bibliography. An unusual feature is the Addendum, which contains brief references to papers of special interest that have been published since the completion of the manuscript. There are items referring to almost every chapter. They stand as a mute reminder of the difficulty of getting a book written and published before it is obsolete in the fast-moving world of modern science. They also suggest that some time in the future a significant revised edition will be issued. The Index of 37 pages is well done. Some will miss an author index, but this seems more than compensated for by the excellent Bibliography.

The book is well written and readable. It would be futile to search it for errors, since almost every statement of fact is documented. It is a conspicuous addition to the growing literature on the liver. Unfortunately, its great weight (4 lbs. 15 oz.) does not lend itself to casual reading, and its high cost may put it beyond the purse of the general student.

J. Walter Wilson

ANIMAL PHYSIOLOGY

Symposium on Antibodies: Their Production and Mechanism of Action *given at a Research Conference for Biology and Medicine of the Atomic Energy Commission sponsored by The Biology Division, Oak Ridge National Laboratory, Oak Ridge, Tenn. held at Gatlinburg, Tenn., April 8–10, 1957. J. cell. comp. Physiol., Vol. 50, Suppl. 1.*

Edited by A. Hollaender. The Wistar Institute of Anatomy and Biology, Philadelphia. Free upon request (paper). x + 361 pp.; ill. 1957.

This collection of 16 papers presented at the conference provides an exceptionally complete, informative, and thought-provoking review not only of basic immunology but also of immunology as applied to the problem of bone marrow transplantation. Although the symposium grew out of the interest in the Oak Ridge Biology Division in transplanting bone marrow to promote radiation recovery, it was decided not to concentrate all of the discussion on the applied problems, but instead to examine the background of the immunological concepts bearing on them. Consequently two-thirds of the contributions deal with the characterization of antibodies and their synthesis and function. Follow-

ing a general introduction by W. H. Taliaferro (Synthesis and Degradation of Antibody), are papers by F. J. Dixon on Characterization of the Antibody Response, S. J. Slinger on Physical-chemical Studies on the Nature of Antigen-Antibody Reactions, and an especially lucid account of Size and Heterogeneity of the Combining Sites on an Antibody Molecule, by E. A. Kabat. The article by D. W. Talmage on Diversity of Antibodies is noteworthy also. Concepts of protein synthesis were treated in relation to antibody formation by G. D. Novelli and J. A. DeMoss, who considered chiefly amino acid activation, and by R. S. Schweet and R. D. Owen, who reviewed theories of antibody formation. The latter authors favor the argument that "antibody formation involves a unique provision for modification by antigen of the heritable equipment of particular cells that can make globulins." They select an initial effect of antigen on nuclear deoxyribose nucleic acid (DNA) as the simplest possibility. Given a heritable change produced in DNA by an antigen, and the consequent development of new ribonucleoprotein templates, a second action of antigen was suggested, very similar to the action of inducer in adaptive enzyme synthesis. The discussion following the paper is effective in pointing up the weak points of the hypothesis. Another paper of uncommon interest is that by R. W. Wissler, F. W. Fitch, M. F. LaVia, and C. H. Gunderson who, after reviewing the cellular basis for antibody formation, conclude that the *exclusive* role of the macrophage, the lymphocyte, or the plasma cell in the antibody-forming process is not supported by existing evidence. For example, they believe that the transformation to the mature plasma cell is but one of the possible end stages of the antibody-forming cell. They argue that the principal antibody-forming cell appears to be a large *pyroninophilic* cell derived by mitosis from a primitive fixed reticular cell whose phagocytic powers are at best limited. This cell type may transform ultimately into a small lymphocyte or a plasma cell. Little attention is paid to the question whether two cell types may be required, the first or phagocytic type transferring the stimulus to a second, globulin-producing, cell. Two papers deal with cellular immune reactions, N. A. Mitchison's on Adoptive Transfer of Immune Reactions by Cells, and E. L. Becker's on Hypersensitivity.

Against this background are 6 comprehensive papers dealing with bone marrow transplantation, including 2 by T. Makinodan and 2 by the Harwell group, including C. E. Ford, P. L. T. Ilbery, J. F. Loutit, D. W. H. Barnes, and P. C. Koller. The other papers are those of C. C. Congdon, and of D. W. van Bekkum and O. Vos. Emphasis is placed on immunologic aspects of the subject, the genetic implications being given less prominence. It must be noted, however, that out of this conference grew the succeeding Symposium on *Genetic Approaches to Somatic Cell Variation*, which has already been published.

JAMES D. EBERT

THE MOTILITY OF MUSCLE AND CELLS.
By Hans H. Weber. Harvard University Press, Cambridge. $3.50. vii + 69 pp.; ill. 1958.

This book contains three lectures originally given as the Dunham Lectures at Harvard University in 1957. Weber presents not only factual material which bears on the chemical basis of contractility in striated muscle and the motility of a number of unicellular organisms, but he also enunciates a theory of the process by which chemical energy is transformed into work during a contractile event. The factual material is presented in a singularly clear and concise fashion. This is particularly true of the techniques which Weber and his colleagues have used in studying the movements of various cells. Certainly there will be many who study muscle—this reviewer among them—who will not grant Weber the privilege of presenting only those facts which are essential to the picture which he has in mind, while dismissing all others with the opinion that they are not contradictory. Recent developments in muscle physiology have raised so many questions concerning our basic concepts that it is difficult to know what to retain and what to reject. Neither will everyone agree with the arguments (p. 5) that studies on living muscle do not help to answer the question of whether or not adenosine triphosphate (ATP) has any connection with the contraction cycle. At the present time this is an area of active research and several studies have been reported that bear precisely this point. The impossibility of showing ATP splitting during contraction is by no means as firmly established in the minds of others as it is in the mind of Weber. Furthermore, the experimental findings which first led to the view that ATP was involved in muscular contraction were obtained from studies on living muscle combined with studies on muscle extracts. I refer of course to the work of Lundsgaard and Lohmann.

It is Weber's view that the ideal material to use in the study of muscular contraction is the isolated contractile system produced by glycerination. It is certainly true that studies on glycerated muscles have strengthened the view that ATP is a direct energy source for the contractile machine. But it is also true that much is yet to be learned about the contractile mechanism from electron microscopic, physiological, and biochemical investigations on whole muscle and on single fibers as well as fiber models and extracts.

Finally, the theory of the contractile mechanism which Weber advances deserves a comment. It is not at all clear how Weber's theory is dictated by the facts which he presents, and it is even less clear exactly how and what characteristics of contraction the theory explains. Neither does Weber explicitly state the molecular origin of the contractile force, that is, whether it is due to entropic effects, electrostatic interaction, or what. In short, this book will serve as an excellent survey of the experimental work which has been done on *glycerinated* material.

FRANCIS D. CARLSON

ÉTUDE DE LA FONCTION THYROIDIENNE AVEC L'IODE RADIOACTIF.

By A. Vannotti. Benno Schwabe, Basel; Intercontinental Medical Book Corporation, New York. $6.50. 203 pp.; ill. 1957.

The use of radioactive iodine (I^{131}) as a tool in the diagnosis of thyroid disease has uncovered some very subtle regulatory mechanisms. A. Vannotti, of the University of Lausanne, has summarized his studies in this excellent monograph. The following determinations are used by Vannotti and his associates for the study of thyroid function: basal metabolic rate, level of cholesterol in the blood, level of protein-bound iodine in the plasma, curve of uptake of radioactive iodine, and conversion rate. The last two methods require some comment. The author measures the uptake of I^{131} by the gland after 1 hour, 3 hours, 24 hours, and 48 hours. These enable him to construct an uptake curve, which is far more informative than the result of one determination performed 24 hours after administration of I^{131}. For the determination of the conversion factor the author measures the protein-bound I^{131} and the total I^{131} in the plasma 24 hours after administration of I^{131}. The index of conversion is calculated by the formula

$$\frac{\text{Counts/minute protein-bound } I^{131}}{\text{Counts/minute total plasma } I^{131}} \times 100.$$

This battery of tests provides the author with a great number of interesting results. Their analysis leads to an excellent insight into the mechanisms governing thyroid function. For instance, severe liver damage affects both the destruction and the elimination of thyroid hormones profoundly. Some cases of low metabolic rate are recorded with an apparently normal thyroid. Their uptake curve and protein-bound iodine in the plasma are normal. These patients do not respond to administration of thyroxin, but respond to administration of triiodothyronine with a rise in metabolic rate. This is interpreted as a failure of the tissues to respond to thyroxine and to convert it into triiodothyronine.

In other patients, the thyroid does not produce thyroxin, but instead produces hormonally inactive iodine compounds, e.g. monoiodotyrosine and diiodotyrosine. This leads to hypothyroidism associated with normal or even elevated values for the protein-bound iodine of the plasma. This anomaly is interpreted as a consequence of a derangement of the enzyme pattern of the thyroid gland.

The references are well chosen from the world literature. The book is warmly recommended to physiologists and physicians with a knowledge of the French language.

W. FLEISCHMANN

REPRODUCTIVE PHYSIOLOGY. *Comparative Reproductive Physiology of Domestic Animals, Laboratory Animals and Man.*

By A. V. Nalbandov; drawings by Evan Gillespie. W. H. Freeman & Company, San Francisco. $6.75. xii + 271 pp.; ill. 1958.

This book is the outcome of a course of lectures to university students having a great variety of biological backgrounds. "Trying out" the text in this manner has helped the author to produce a treatise easily understood by the general reader. The excellent balance of the contents is the result of its unified authorship, not possible in the symposium type of book. Hiatuses in our knowledge of the subject are pointed out. Indeed, reproductive physiology is not presented here as a static science, but the reader is made to feel that the subject is as viewed by one writer of 1958.

The treatment of the anatomy and histology of the gonads and of the male and female genital tracts and accessories, including those of birds (chickens), affords an adequate background for physiological discussions, such as the pituitary-gonadal relationships and the concomitant phenomena of the estrous and menstrual cycles. An especially competent chapter is that on the Chemistry and Physiological Properties of the Hormones, written by the author's wife, a doctoral graduate in chemistry. The biology of eggs and spermatozoa is treated comprehensively and with the usual compactness, and the relations of their characteristics to problems of fertility and sterility is adequate to orient the reader in an important aspect of the subject. Hormonal action on the target organs is noted throughout, including the maintenance of pregnancy and lactation.

While the book was mainly written for the prospective animal husbandman, the doctor of medicine too will find suggestions herein, though little relating directly to the human species.

The book is amply illustrated with well-selected photographs and easy-to-read diagrams—with one

exception, the chart on p. 135, which I must confess that I do not understand.

The 7-year estimate of the survival of spermatozoa in the honey-bee queen (p. 184) may need revision downward, as there is some genetic evidence that the queen may mate with her sons in the hive.

The binomial method of numbering the illustrations, in which the number of the chapter is followed by the number of the figure, seems unnecessarily cumbersome. In these days one finds few misspelled words in the printed pages of books; but on p. 186 the great Hertwig would have preferred an *e* in place of an *a*; and the Greek scholar would like an *o* instead of an *e* in oophorus (p. 138). By the way, oviferous is correct but cacaphonic; ovigerous is used in reference to certain lower forms that at times carry their eggs and embryos around with them.

For many types of college courses this volume fills the need, either for a text in special courses or as collateral reading in more general courses of study.

CARL G. HARTMAN

RECENT PROGRESS IN THE ENDOCRINOLOGY OF REPRODUCTION. *Proceedings of the Conference held in Syracuse, New York, June 9–12, 1958.*

Edited by Charles W. Lloyd. Academic Press, New York and London. $12.00. xiii + 532 pp.; ill. 1959.

This well-known series needs no introduction to investigators in the field of reproductive endocrinology. One may properly regret that "recent" refers to papers written more than a year before the publication of the present volume! Twenty-three chapters, written by recognized authorities, discuss in detail various aspects of the role of the nervous system, the effects of environment, the metabolism of hormones, and their relation to the stages of reproduction. Each chapter contains numerous hitherto unpublished observations. The editor should be complimented on excellent reporting of the discussion occurring at the conference. The comments have been limited to pertinent and coherent material, and even include tables of experimental results.

DAVID E. DAVIS

METABOLISM OF THE NERVOUS SYSTEM. *Proceedings of the Second International Neurochemical Symposium held at Aarhus, Denmark.*

Edited by Derek Richter. Pergamon Press, New York, Los Angeles, Paris, and London. $16.00. xiv + 599 pp.; ill. 1957.

This is a pleasing example of international, cooperative scientific effort. The subject matter of the Symposium, held in 1956, is divided into 13 basic areas covering a very broad range. Such topics are included as molecular structure, chemical composition, permeability, electrolytes and nervous conduction, metabolic studies in vivo and in vitro, energy metabolism, the metabolism of lipids, nucleic acids, and proteins, cholinergic and non-cholinergic transmission, and other pharmacologically active compounds. Each subject is prefaced with a short introduction and is comprised of 2 to 7 subtopics or papers followed by properly subordinated discussions pertinent to each paper. Although this book is not meant for use as a textbook, its broad scope and extensive coverage of recent advances in the physiology, biochemistry, and morphology of nervous tissue metabolism appear to qualify it as a good, up-to-date, graduate text and reference work. The editors and participants of this symposium are to be commended for the well-organized treatment given to this extensive and currently popular area of research.

THOMAS E. NELSON, JR.

FUNDAMENTALS OF NEUROLOGY. *Third Edition.*

By Ernest Gardner. W. B. Saunders Company, Philadelphia and London. $5.95. xi + 388 pp.; ill. 1958.

Fundamentals of Neurology was first published in 1947 to present to students entering a biological field the fundamental principles of neurology upon which more detailed and specialized studies could be readily founded. Now in its third edition, this widely used college textbook has been revised to place a greater emphasis on the sensory endings and to include a new, short introductory chapter to neurochemistry. Superficially, there appears to be little change in the book's general format or context; however, that impression is quite misleading. Although certain sections have not been changed at all, others have been extensively revised. The terminology used complies for the most part with the Nomina Anatomica approved in 1955. The third edition should continue to keep this book in a foremost position among preclinical neurological texts.

THOMAS E. NELSON, JR.

THE PHYSIOLOGY AND PATHOLOGY OF THE CEREBELLUM.

By Robert Stone Dow and Giuseppe Moruzzi. University of Minnesota Press, Minneapolis. $12.50. xvi + 675 pp.; ill. 1958.

The cerebellum is a part of the brain which has attracted investigation for more than a hundred years, and over much of that time interest in it has been lively. This structure includes the second great

cortical area of the mammalian central nervous system, and it has shown a great increase in size and importance in its phylogenetic development. It sits astride the ancient and life-essential mechanisms of the lower brainstem, it receives important projections from all of the peripheral receptors and from almost all parts of the brain, and its efferent pathways have an enormously wide distribution. During the past twenty years, particularly, many observations on cerebellar anatomy and physiology have been published and have vividly supplemented and altered our concepts on its role of integration of nervous activity.

During this recent period, the cerebellum has received several outstanding monographs, of which this superlative work is the most recent. It is more than appropriate that one of the authors of the work under review should be Giuseppe Moruzzi, because of the outstanding contributions to cerebellar physiology which have come from Italian scientists during the last one hundred years. Not the least of the services rendered by this work is the summary and critical appraisal of the Italian literature, not easily accessible physically or linguistically to many American workers.

It is difficult to express adequate appreciation for the sheer amount of work and exacting care which Dow and Moruzzi have put into this volume. However, it is much more than a compendium: it is a critical and stimulating analysis of both classical and contemporaneous neurophysiology. Recent contributions, some previously unpublished, of the Pisa laboratory are appropriately included in detail. In short, this work illustrates vividly that the more dynamic and valid textbooks are written by those actively engaged in the field they are describing, rather than by the passive observers.

More than fifty years have passed since Sherrington stressed the integrative nature of nervous function. Since then the cerebellum has outgrown its designation as the "head ganglion of the proprioceptive system," and this book shows well how intricately interwoven is its function with that of many, if not all, sensory and motor systems, with the cerebrum, and with the reticular formation. The emphasis of the present day is again upon nervous interaction, following the new and fascinating unraveling of the many functions of the reticular mechanism. Now it is seen that "sensory" and "motor" systems are much less independent than was once believed and that they modulate one another's activity not only at the cerebral cortex, but at all the classical levels of organization of the brain.

The authors subdivide the enormous mass of neurophysiological material (over 60 pages of bibliography are included) into the following chapters, convenient to the experimenter: Ablation Experiments, Stimulation Experiments, Electrophysiological Experiments, Relations between the Cerebellum and other Central Structures, Developmental Physiology, and General Considerations on the Function of the Cerebellum.

The material covered in this volume is too large to permit even enumeration, much less a critical appraisal in this review, a task which should be reserved for the specialized journal. Certain major points will be noted about the neurophysiological parts of this volume; the chapters on clinical symptomatology and pathology will not be so covered, not because of any lack in their importance, but because they lie outside the competence of the reviewer.

The authors state that "there have been three principal sources of error throughout the entire history of cerebellar investigation." These are "the failure to recognize differences between species . . . , the failure to appreciate which symptoms are the result of acute loss of cerebellar influence on the rest of the nervous system and which are due to other centers correcting or compensating for this loss . . . , and the lack of histological controls."

Ablation experiments have led to the conclusions that in the bird the cerebellum is mainly concerned with the regulation of spinal and brainstem mechanisms, in the cat and dog it is rather equally devoted to spino-brainstem and to cerebral mechanisms, and in primates it is concerned chiefly with voluntary movements. The authors emphasize the primary concern of the cerebellum with the somatic motor mechanism: (a) postural tonus, (b) reflex phasic contractions, and (c) voluntary phasic contractions. They point out the essential difference between the reflex excitability of and the strength of postural mechanisms, and that these are often affected in different ways by cerebellar ablation. They stress the fact that the intensity and type of deficiency symptoms are not necessarily the same following total or unilateral cerebellectomy. Deficits are not limited to the side of unilateral cerebellar lesion. Moreover, the cerebellum is not a functionally homogeneous structure, and both inhibitory and facilitatory influences are executed on a wide sphere of activity.

There is a very thoughtful discussion of the effects of electrical stimulation of various parts of the cerebellum, and of the ensuing phenomenon of rebound. Work in the Pisa Laboratory has contributed significantly to dissecting the role of the vermal cortex and the underlying fastigial nuclei, and has confirmed the general distinctiveness of this part of the organ.

One of the fascinating characteristics of the electrical activity of the cerebellum is the fast rhythm recorded from its cortex, a rhythm very different

from that found in the cerebral cortex. This cerebellar activity appears to be largely self-generated, in that it persists with little change after complete severance of incoming nervous connections. There is no powerful extrinsic synchronizing force, and the cerebellar rhythm does not change in the arousal process which is so important to the activity of the thalamus and cerebral cortex. The conclusion that the cerebellar cortex is very differently organized from the cerebral is again symphasized in that evoked epileptiform discharges do not spread to distant areas, but are confined to the site of stimulation. Local application of strychnine excites the cerebellum without causing the paroxysms of hypersynchrony so characteristic of the cerebrum.

A recent finding of considerable interest is the role of the cerebellum in controlling both alpha and gamma motor cells, in regulating the intensity and the spatial and temporal relations of the firing of these motoneurons. The rigidity which follows decerebration is caused by an increase in the gamma firing which increases the magnitude of the stretch reflex by discharging the muscle spindles. Removal of, or physiological interference with, the cerebellum (chiefly its vermal part) diminishes the gamma firing, but greatly increases the activity of the alpha motoneurons. This increase results in decerebellate rigidity, which is caused by a release of the vestibular mechanism from tonic inhibition from the cerebellum. Thus revealed are the two components of postural adjustments, the peripheral or myotatic, and the central, spinal and vestibular.

This book is a work of devoted scholarship and is highly recommended to all neuroanatomists, neurophysiologists, and neurologists interested in the central nervous system.

JAMES M. SPRAGUE

ANIMAL NUTRITION

ADVANCES IN FOOD RESEARCH. *Volume VIII. Edited by E. M. Mrak and G. F. Stewart. Academic Press, New York and London.* $12.00. xii + 437 pp.; ill. 1958.

This volume contains 6 reviews, 3 of which should be of interest to many plant physiologists as well as to the food researcher. A. C. Hulme (Larkfield, England) has attempted to "present the overall biochemical and physiological picture of the apple and pear as it is today." The author's goal is achieved in a 116-page discussion of the changes that occur in the fruits during maturity and storage. M. A. Amerine (Davis, California) has done an excellent job of cataloging data from over 500 references on the inorganic constituents of wines. An earlier review on the organic constituents of wines (*Food Research*, Volume 5) is also extended here by inclusion of several compounds not previously discussed. Of general interest are the author's comments on the analytical methods used for analysis of cations and anions in wines. Quantities, sources, and effects of the inorganic constituents are also included. P. A. Roelofsen (Delft, Netherlands) describes the fermentation, drying, and storage of cacao beans. Microbiological and enzymatic aspects of the problem are emphasized. F. A. Lee (Geneva, New York) reviews the changes in physical and biochemical properties encountered with various blanching processes. An excellent section discusses changes which occur during frozen storage of unblanched material. Bacteriological and engineering aspects of blanching are not covered in detail. M. Milner (Manhattan, Kansas) condenses developments of the last ten years in the detection of infestation in grain and of the separation of infested kernels from sound kernels. Rheology in Food Research, a review by G. W. Scott Blair (Reading, England), treats of the use of physical means to measure the functional characteristics, or to estimate the subsequent properties, quality, or acceptance of food products. This review should both stimulate work on the development of more adequate methods for making rheological measurements and also serve as "food for thought" for those currently utilizing such techniques.

PAUL A. HARTMAN

PROCESS ENGINEERING IN THE FOOD INDUSTRIES. *By R. J. Clarke. Philosophical Library, New York.* $10.00. viii + 355 pp.; ill. 1957.

The title page of this book indicates that the text was written for those who are responsible for the management of food-processing plants, chemists, and engineers entering the food industry. As such, it is organized about the various engineering operations which might be encountered in food processing. Each operation is considered in a separate section or chapter. A brief statement is given of the principles behind such operations, and then the engineering equations for the operation are considered. Finally, the section is concluded by a consideration of the machinery used in the particular operation as well as the application of the operation to various commodities. The latter sections are purely descriptive.

It is unfortunate that in the proof reading of a book of this type a number of errors escaped correction. If errors were confined to the descriptive material, one might overlook them; but in general these are errors in the engineering formulas given at the beginning of each section. An example may be found in the introduction, where as an example

of dimensional analysis the dimensions of the overall heat transfer coefficient are given. The dimensions which are cited, however, are not those of the heat transfer coefficient but those of mass velocity. The arrangement of the engineering formulas and equations is not consistent. Sometimes the symbols of the equation are defined, at others they are not. While this would be of no great consequence to an engineer, the chemist or manager would probably encounter difficulties thereby.

The author evidently wrote primarily for British readers. All of the machinery illustrated is of British manufacture, and the processes which are described are those used in Great Britain and in many cases differ from those used elsewhere.

In a book of this type written primarily from an engineering viewpoint, it might be unjustified to criticize the author's failure to consider the effects of processing on the nutritive value, flavor, color, and appearance of foods, but it would seem that if one deals with a food commodity, at least some consideration should be given these aspects.

C. O. CHICHESTER

BIOPHYSICS AND GENERAL PHYSIOLOGY

ADVANCES IN RADIOBIOLOGY. *Proceedings of the Fifth International Conference on Radiobiology held in Stockholm on August 15th–19th, 1956.*

Edited by George Carl de Hevesy, Arne Gunnar Forssberg, and John D. Abbatt. Charles C Thomas, Springfield, Ill. $15.00. xvi + 503 pp.: ill. 1957.

This symposium volume is a cross-section of progress in radiobiology. Appropriately, the volume is dedicated to H. J. Muller.

Recent advances in this field are impressive and indicate the efforts of an international group of dedicated workers. It is more appropriate here to mention active fields rather than individual contributions, and the following are among the forward steps being taken: radiation protection with SH-compounds, transplantation of normal bone marrow into irradiated animals, irradiation of single cells and parts of cells with alpha particles, radiation and aging, effects of radiation on the haemopoietic system, and studies of the induction of detrimental mutations. No single paper provides a strikingly new insight into the radiation problem of damage. Instead the overall impact is one of ever-narrowing segments of study on the manifold aspects of a complicated phenomenon. However, the continuing degree of sophistication of radiation research is evidence that the various facets are becoming of manageable proportions, and that steady progress in delineated sectors is to be expected.

C. P. SWANSON

BIOCHEMISTRY

CHEMICAL TRANSFORMATIONS BY MICROORGANISMS. *E. R. Squibb Lectures on Chemistry of Microbial Products.*

By Frank H. Stodola. John Wiley & Sons; Chapman & Hall, London. $4.25. x + 134 pp. 1958.

The title of this book is more encompassing than the small 134-page book itself. The book is actually a series of three lectures by F. H. Stodola of the Northern Utilization Research and Development Laboratory of the United States Department of Agriculture. This is the second series of E. R. Squibb lectures on Chemistry of Microbial Products at the Institute of Microbiology at Rutgers University.

The three lectures make up 3 chapters as follows: The Chemical Anatomy of Microorganisms, with Special Reference to *Mycobacterium tuberculosis*; Organic Type Reactions of Microorganisms; and Synthetic Power of Microorganisms. Although this is a small book, and is actually even shorter than would appear because many pages are taken up by diagrams of steroid compounds and other structural formulas and equations, it is an extremely interesting and well-written book with considerable valuable information. The author has wisely made no attempt to try to cover all of the reactions that might be discussed. Instead, he has listed the general type reactions and given brief mention to most of them, and has saved most of his space for special subjects in which he himself has made research contributions.

In the first chapter the author describes how the organic chemist has aided the microbiologist in attaining a better understanding of the functioning of microorganisms through a study of their chemical composition. In the second chapter he illustrates how the microbiologist has reciprocated by providing the organic chemist with microbial tools for better reactions than could be provided by purely synthetic methods alone. This he illustrates by means of 22 different examples from the literature showing how it is possible to introduce single hydroxyl groups into most of the assailable positions of the steroid molecule by the use of different microorganisms.

He classifies the type reactions in microbiological chemistry into 15 divisions. These are: oxidation, reduction, hydrolysis, decarboxylation, deamination, amination, phosphorylation, dehydration, con-

densation, methylation, dismutation, amidation, esterification, acylation, and transglycosylation.

The last chapter, The Synthetic Powers of Microorganisms, deals with certain pigments, zymonic acids, α-ketoglutaric acid and the gibberellins. In this chapter antibiotics are mentioned, but the author admits that he will make no attempt to present that subject and merely recalls two interesting historical incidents dealing with the development of penicillin.

The historical developments of organic chemistry and of microbiological chemistry are presented very well and make interesting reading. The author gives careful attention to experimental methods used by different workers, for example, the methods that have made it possible to obtain a chemical analysis of cell wall material, flagella, and cytoplasmic membranes. Although his short book contains almost 300 references, it is no mere review of the literature. The author comments critically and points out further problems, as for example where he states, "Temporarily we are halted, for no way is yet available for separating the different types of granules and determining their composition and functions" (p. 27).

S. S. Block

The Metabolism of Sulphur Compounds.
By Leslie Young and George A. Maw. Methuen & Co., London; John Wiley & Sons, New York. $3.00. 180 pp. 1958.

A real credit to the series of Methuen's Monographs, to which it is a recent addition, this little volume attempts to summarize the entire field of sulfur metabolism in higher animals and microorganisms, and comes very close to succeeding. Subscribing to the familiar format of its predecessors in the series, the volume is organized as a series of short chapters each with well-delineated attention. The text is well documented, with the pertinent references appearing at the end of each chapter. A fine subject index completes the system of organization which enables the reader quickly to locate and refresh his memory upon any subject encompassed within the volume. Equally of note is the fact that the authors have achieved a concise presentation of a large amount of information and yet have avoided the dry, textbookish tone all too frequently encountered in undertakings of this nature.

The volume begins with a brief introduction to the types of sulfur compounds encountered in biological systems and a short general discussion of experimental approaches which have been particularly fruitful in studies of sulfur metabolism. Following this are 6 chapters dealing with a physiological and metabolic consideration of the organic sulfur-containing compounds in higher animals. The sulfur-amino acids and their metabolic products, transmethylation, the sulfur-containing vitamins and coenzymes, sulfomucopolysaccharides and other sulfate esters, and several compounds of pharmacological and therapeutic import are among the subjects considered in these chapters. The picture for higher animals is completed by a discussion of the metabolic significance and fate of inorganic sulfur compounds.

A final chapter, entitled Microbial Aspects of Sulfur Metabolism, encompasses such subjects as sulfur-amino acid synthesis and degradation, sulfur nutrition, symbiotic relationships involving intestinal and rumen organisms, and sulfur metabolism in autotrophic bacteria. If the volume has a deficiency, it is that the subject matter of this chapter was not developed more fully.

This volume is a valuable addition to any personal library of biochemistry. Its low cost will be instrumental in putting it there.

K. J. Monty

Progress in the Chemistry of Organic Natural Products. *Volume XV.*
By J. L. Hartwell, D. C. Hodgkin, H. H. Schlubach, A. W. Schrecker, and L. Zechmeister. Springer-Verlag, Wien. $9.75. vi + 244 pp.; ill. 1958.

Progress in the Chemistry of Organic Natural Products. *Volume XVI.*
By J. Bonner; 6 contributors. Springer-Verlag, Wien. $9.50. vi + 226 pp.; ill. 1958.

Volumes XV and XVI of this important publication contain again a series of excellent articles. Volume XV is introduced by Schlubach with a short review on the carbohydrate metabolism of grass. In the next article Zechmeister, the editor of the series, reports on conversions of carotenoids brought about by the action of bromosuccinimide or boron trifluoride. Dehydration of the colorless substances phytoene and phytofluene with bromosuccinimide led to their identification with hydrogenated lycopene derivatives. Eighty pages of the volume are devoted to an article by Hartwell and Schrecker on the chemistry of *Podophyllum*, which has been recommended as an inhibitor of tumor growth but does not seem to have significant therapeutic value. The volume closes with an outstanding article by Dorothy Hodgkin, in which she tells the exciting story of her brilliant x-ray analysis of the structure of vitamin B_{12}. Numerous diagrams and electron density maps enable the reader who is not experienced in this field to understand the experimental approach and the principles involved in this powerful method of structural analysis.

Volume XVI contains three articles on the structure of organic natural products. The first deals with catechol and other derivatives of hydroxylated flavans, the second with *Aconitum* and *Delphinium* alkaloids, and the third with the antibiotics produced by actinomycetes. Among the 24 antibiotics described by van Tamelen are actinomycin, aureomycin, chloromycetin, erythromycin, magnamycin, streptomycin, terramycin, and other important substances of this group. In a subsequent article protein synthesis in plants is discussed by J. Bonner. In the last article of the volume the electron gas theory of the color of dyes is explained by its proponent, H. Kuhn. The resonating systems of polyenes, cyanins, porphyrins, and other dyes of biological interest are illustrated by typical formulas and models. Both volumes are highly recommended as valuable source books for biologists and chemists.

F. Haurowitz

The Chemical Analysis of Foods and Food Products. *Third Edition.*

By Morris B. Jacobs. D. Van Nostrand Company, Princeton, Toronto, London, and New York. $13.75. xxii + 970 pp.; ill. 1958.

This book is the third edition of one originally printed in 1938. The second revision occurred in 1951. The relative lengths of intervals between editions are no doubt indicative of the rapidity with which new analytical methods are being developed. Undoubtedly Jacobs, in 1938, did not then envisage the addition of a chapter on Radio Chemical Determinations to his book.

The book consists of 24 chapters covering the analysis of food and food products. Specific directions for analytical procedures are given for different compounds of interest to the food chemist. At least in part, the book is a manual which can be used for routine chemical analysis. The volume differs from the *Methods of Analysis* of the Association of Official Agricultural Chemists in that it covers methods which are not considered official and omits some which are not of interest to the food chemist. By restricting the content of the volume to a smaller range of products, space is provided here for some general discussion of methods. For example, the book contains a chapter on flavor evaluation by means of a subjective method which is certainly not considered by the A.O.A.C.

In the 1958 edition three chapters have been added which do not appear in the 1951 edition: Radio Chemical Determinations, Pesticide Residues, and Artificial Sweetening Agents. The earlier chapters, as judged by the literature references, were revised extensively in 1951, then reviewed again in 1958.

The book should be extremely valuable to any chemist concerned with the food industry, and particularly to individuals who are working in the field of quality control of food and food products.

C. O. Chichester

MICROBIOLOGY

Problèmes d'Organization et de Fonctions chez les Bactéries et les Virus.

Edited by J. André Thomas. Masson & Cie., Paris. 5,000 fr. (paper). xii + 386 pp.; ill. 1958.

It is unfortunate that J. A. Thomas did not confine his symposium to the viruses. Prévot's uncritical review of notions regarding bacterial structure, cytochemistry, and classification is an incomprehensible hodgepodge; and Hauduroy has added nothing new to his previous discussions of filterable forms of bacteria. While making a pretense of being historically correct and up to date on the L cycle of bacteria, the result is a second very incomplete and uninformative review.

Only R. Vendrely's review of nucleic acids and nucleoproteins does the bacteria justice. Giroud and Dumas review very well the little that is known concerning the Rickettsiales. A chapter on the crystallography of viruses, by Barraud, emphasizes the historical development of our current views of virus structures as obtained by crystallography and substantiated by other techniques. The best article in the volume, the one by C. Hannoun, discusses in very general form some principles involved in the elucidation of the antigenic constitution of viruses and proceeds to brief and clear descriptions of specific examples. The final article, by Lépine, reviews electron microscopic observations on virus-infected animal cells before rambling off into a wilderness of theory concerning virus multiplication.

Philip E. Hartman

The Cellular Slime Molds.

By John Tyler Bonner. Princeton University Press, Princeton. $4.00. viii + 150 pp. + 8 pl.; text ill. 1959.

One of the most unusual, almost unbelievable, groups among all organisms is that of the slime molds. The fact that the single, autonomous slime mold cells are able to come together and to form a single unified body, which proceeds thereafter to behave as a delicately integrated organism, tantalizes the imagination. In the group of slime molds known as the Acrasiales, the amoebae retain their morphological individuality within the aggregated

pseudoplasmodium; hence, the title of this book on the Acrasiales is *The Cellular Slime Molds*.

Virtually everything that is known about all of the Acrasiales is summarized in this little book. After first succinctly distinguishing the Acrasiales from the other three groups of slime molds, the Myxomycetales, Plasmodiophorales, and Labyrinthulales, Bonner proceeds to characterize the 8 known genera. Illustrations of their fruiting body structure and some comments on their frequency in nature are included.

The remainder of the book is devoted to a careful description and discussion of the life cycle of *Dictyostelium discoideum*, which is contrasted with that of the other closely related species and genera upon which experimental work has been done. Included, along with observations from Bonner's own years of research on the slime molds, are the major observations, carefully credited, made by the many other excellent workers in this field. The aggregation and the migration of the slime mold are examined by means of a comparison with examples of morphogenetic movement in other organisms and, wherever possible, an attempt is made to correlate experimental observations with speculation about the controlling mechanism. Bonner includes here a cogent discussion of the evidence for and against the occurrence of sexuality in the order, and concludes that true fusion and meiosis have yet to be demonstrated among the Acrasiales. A final section of the book is concerned with cell variation, between species, within a strain, and within a clone, and the possible bearing this has on the differentiation of prespore and prestalk cells within a single slug.

Written to summarize and discuss current knowledge of the Acrasiales, this book contains a minimum amount of technical language and yet does not neglect the interesting details of the research. The photographic plates, collected from several investigators, are superior. They include several electron micrographs of amoebae, both in the state of single cells and as portions of an aggregate. Two bibliographies are presented, one listing general works cited in the text, the other concerned solely with publications on the Acrasiales, and complete through 1957.

This book is not a monograph, at least in the sense that little mention is given of the natural habitats of the organisms, and nothing of their ranges, if indeed much is known. The taxonomy is but briefly reviewed, although considerable discussion is given to the possible phylogenetic relationships among the organisms discussed. The author's own true interest stirs the interest of the reader throughout: it is the use of the Acrasiales as experimental material for the study of two basic biological phenomena, aggregation and differentiation. The slime molds exhibit these phenomena at what appears to be a very simple level. However, the great number of questions which Bonner raises in his discussion attests to the complexity of even this lowest of molds.

ANNETTE COLEMAN

FOOD MICROBIOLOGY.
By William Carroll Frazier. McGraw-Hill Book Company, New York, Toronto, and London. $9.00. x + 472 pp.; ill. 1959.

Those who have long awaited a succinct, comprehensive presentation of the basic principles underlying food microbiology will welcome this book. The refreshing manner in which W. C. Frazier has condensed present knowledge in an extremely broad area of study will be appreciated by students and workers in microbiology, food technology, and related fields.

The book is composed of six parts, each rather finely subdivided. The first part contains a very well illustrated chapter on each of the 3 major groups of microorganisms important in food microbiology. Discussions of the contamination of foods from natural sources and during processing conclude the first section. A chapter on the general principles of food preservation introduces the second part, and is followed by 5 chapters on specific methods of food preservation and 5 chapters on problems associated with the preservation of various groups of foods and foodstuffs. Part Three is divided into 10 chapters: one chapter on general principles and the remainder on problems peculiar to various types of foods. Following a section of 3 chapters on fermentations of foods, 2 chapters are devoted to foods in relation to disease. The final part includes chapters on food plant sanitation, microbiological laboratory methods, and food control. A compilation of official and recommended microbiological standards is listed in an Appendix.

The integration of the information contained in different parts of the book is left primarily to the reader. This would at first appear to limit the usefulness of the book as an aid to workers in fields related to the food industries; however, the presence of a limited number of judiciously selected references following each chapter and an adequate Index should facilitate the location of more detailed information without confronting the novice with a formidable and confusing array of citations to the literature.

PAUL A. HARTMAN

DAIRY BACTERIOLOGY. *Fourth Edition.*
By Bernard W. Hammer and Frederick J. Babel.

John Wiley & Sons, New York; Chapman & Hall, London. $9.00. ix + 614 pp.; ill. 1957.

As stated in the Preface to the new edition, this textbook "covers the same general material as the earlier editions and, again, the expansion or contraction of certain sections has been based on the increasing or decreasing importance of the subjects involved." New chapters are included on the bacteriology of milk production, milk plant operation, and freezing of milk and cream. Other sections have been rewritten. Such expansions and contractions have resulted in the production of a modernized edition of an old standby. Format, arrangement, and style are essentially unchanged.

Lack of interpretation of detailed information and inadequate selection of references still limit the desirability of the book as a textbook for many beginning courses in dairy bacteriology. For the same reasons, the book should be of considerable value as a text for the advanced student or as a reference manual for the dairy farm and plant operator. A few additional contractions involving limitation of details and more explanation of their implications would have been of help to the student in general bacteriology. A substantial expansion in the other direction would have greatly increased the usefulness of the book as an advanced text and reference work.

PAUL A. HARTMAN

PARASITOLOGY

BIOLOGY OF THE TREPONEMATOSES. *WHO Monogr. Ser., No. 35.*

By Thomas B. Turner and David H. Hollander. World Health Organization, Geneva. $6.00. 278 pp. + 2 pl. 1957.

This monograph is a monumental contribution based on the results of investigations carried out at the International Treponematosis Laboratory Center in the Department of Microbiology of the Johns Hopkins University. The monograph is arranged in three parts: Part I comprises 6 chapters dealing with various aspects of the fundamental biology of the treponematoses in general; Part II, 3 chapters in which a comparative study of strains of treponemes newly isolated from various parts of the world is presented; and Part III, a single chapter summarizing the principal observations presented in Parts I and II.

Altogether, 76 strains of treponemes have been isolated, 70 of these from human beings, and 6 of cuniculi treponemes from rabbits. There were 39 from patients with syphilis, 20 from patients with yaws, 3 from Bejel patients, and 8 from patients with one of the syndromes classified as endemic treponematosis. The authors have found the hamster to be the most satisfactory laboratory animal in which to isolate treponemal strains. Transfers of hamster material to rabbits usually resulted in the establishment of the strain in the latter species. Direct transfer of material from man to the rabbit was also successful in a high proportion of instances. Experience in the attempted isolation of pinta treponemes has been disappointing.

Following testicular or intracutaneous inoculation, syphilis treponemes induce in rabbits an indurated type of initial lesion. Other species of treponemes induce a less indurated type of lesion. Histopathologic changes have been studied, prominent among which is the initial production of a mucoid material identified as hyaluronic acid. Among the developments of better quantitative methods were first, the utilization of a pattern method of multiple intracutaneous inoculation, which has simplified the reading of results and been more reproducible, and secondly, studies on the rate of multiplication of treponemes following rabbit inoculation. It became clear from these studies that after a time lag of 24 to 48 hours treponemes multiply in a logarithmic pattern until interrupted by immune processes, antibiotics, or unfavorable environmental temperatures. The incubation period can be regarded as an index of the number of viable treponemes inoculated. A generation time of 30 to 35 hours for *T. pallidum* can be computed.

The authors have found that the environmental temperature favorable for treponemes is approximately 35°C. Very slightly higher temperatures are unfavorable, and when the temperature rises to near 40°C., progressive destruction of the treponemes occurs. Likewise, as the temperature falls, inhibition of growth is noted. From these investigations has come the realization that the localization of treponemal lesions is influenced, perhaps in a major way, by the local temperature of the animal or human host.

The administration of cortisone to animals infected with syphilis leads to a tremendous overgrowth of treponemes in both initial and secondary foci. Concomitantly there is a great increase in the amount of hyaluronic acid in these lesions. Large numbers of treponemes for experimental purposes can best be obtained from testicular lesions of rabbits following the use of cortisone. A medium which has permitted the survival of pathogenic treponemes for periods up to two weeks has been developed. This development has paved the way for the demonstration of treponemal immobilizing antibody. Studies on long-term survival of treponemes in the frozen state are also presented. The authors have found that when frozen at approxi-

mately −70°C., both syphilis and yaws treponemes have been found to be virulent for rabbits after storage for over 9 years. No reliable method now exists for the cultivation in vitro of pathogenic treponemes.

Immunity in experimental syphilis develops comparatively slowly, at a rate determined in part by the extent of the infectious process, since this determines the degree of antigenic stimulus. The mere presence of infection has not proved to be sufficient to stimulate the development of immunity. Once immunity has developed to its highest point, ordinarily within 3 months after infection, a high level of resistance is usually maintained as long as the infection, even though latent, is present. The persistence of immunity, however, after elimination of infection by therapy seems to be a function of time plus the degree of immunity attained at the time of treatment. Animals in which immunity is fully developed at the time of therapy commonly show no evidence of diminished resistance after one year. The study of treponemes from animals with latent syphilitic infection has failed to reveal biological differences in these organisms which might account for their seeming resistance to the immune defenses of the host.

From this laboratory has come the demonstration that human beings and animals affected with treponemes develop antibodies that are specific for this group of organisms, which led to the development of the treponemal immobilization test that has been widely used both in clinical medicine and in the study of certain fundamental aspects of treponematosis. Considered from a basic biologic standpoint, the T.P.I. test clearly measures an antibody to pathogenic treponemes which differs from the so-called Wassermann antibody. Existing evidence indicates that immobilizing antibody is a highly specific index of past or present treponemal infection. At the clinical level the T.P.I. test has proved to be a noteworthy aid in the management of treponemal infection. It has been a valuable means for identifying the so-called biologic false-positive Wassermann reactors.

Because of the complicated technical features of the T.P.I. test, intensive search has been made for other methods of detecting specific antibodies. In particular, study of the phenomenon of treponemal agglutination as a specific antigen-antibody reaction has been undertaken.

In the authors' laboratory the response of treponemes to antibiotics has been extensively studied by both in vivo and special in vitro methods. Assays of some of the newer antibiotics showed that while none was nearly as effective as penicillin, a number did have significant therapeutic potentialities against the Nochols strain of *T. pallidum*. Magnamycin was the most effective. Attempts to induce increased resistance to penicillin under experimental conditions have given no indication that such a phenomenon does or can occur.

The authors have also studied extensively the comparative characteristics of experimental disease invoked by various strains of treponemes. The pathogenic treponemes which they have been able to study in the laboratory have proven to be very closely related in their essential biological characteristics, in the disease picture they invoke in man and in experimental animals, in their immunological features, and in their reaction to antibiotics. However, certain relatively stable differences have been observed particularly in relation to the kind of lesions invoked in rabbits, the disease picture in hamsters, and certain immunological patterns as determined by challenge inoculation of rabbits. On the basis of these criteria, strains of treponemes from various parts of the world have been placed, with some unexplained overlapping, into one of three categories: the syphilis or S type, comprising most but not all of the strains isolated in patients with the classical disease syndrome of venereally acquired syphilis; the yaws or Y type, comprising most but not all of the strains isolated from patients with the classical disease syndrome of yaws; and the M type, which in the foregoing characteristics occupies an intermediate position between the syphilis and yaws types and which comprises most but not all of the strains isolated from patients with disease syndromes of bejel, endemic syphilis, and locally designated syndromes of endemic treponematosis, such as dichuchwa.

This monograph should prove invaluable to biologists as well as to physicians.

RICHARD D. HAHN

HEALTH AND DISEASE

ANNUAL REVIEW OF MEDICINE. *Volume 9. Edited by David A. Rytand and William P. Creger. Annual Reviews, Palo Alto.* $7.00. x + 530 pp.; ill. 1958.

The ninth volume of the *Annual Review of Medicine*, 1958, continues its tradition of presenting current evaluations and literature citations in selected areas of medicine. One of the exciting developments of modern medicine has been the closer integration of clinical medicine with the more basic biologic sciences. This point of view is reflected by the majority of the articles, which range from Viruses to Dentistry. Of particular interest to biologists may be reviews such as Autoimmunity in Disease, by F. J. Dixon; Psychopharmacology, by J. D. French; and

Toxicology of Radioactive Materials, by H. Foreman. The application of modern techniques of experimental design and their most efficient use in patients is illustrated by L. Lasagna and P. Meier in Clinical Evaluation of Drugs. Short articles on the application of chromatographic, electrophoretic, and enzyme techniques to medicine are helpful. Brinkhous et al., in a scholarly paper, review the rapid progress made in hemophilia research recently and discuss in detail some of the newer coagulation factors.

The *Annual Review of Medicine* continues to be of most value to physicians and particularly to medical university teachers who can thereby rapidly keep up with the most significant advances in fields other than their own specialization. However, these review articles also give the non-medical biologist an excellent grasp of the vast amount of research conducted in the many areas related to clinical medicine.

ARNO G. MOTULSKY

CRYPTOCOCCOSIS. *Torulosis or European Blastomycosis.*

By M. L. Littman and Lorenz E. Zimmerman. Grune & Stratton, New York and London. $8.50. x + 205 pp. + 4 pl.; text ill. 1956.

The monograph, *Cryptococcosis*, can be recommended without reservation as an authoritative, critical, thorough, readable, and beautifully illustrated reference work. It reviews competently all aspects of a mycosis which equals coccidioidomycosis and histoplasmosis in medical importance and probably has been confused with histoplasmosis in some epidemiological studies. The combined training and experience of the authors in mycology, clinical medicine, and pathology, and the rich mycological resources in the files of the Armed Forces Institute of Pathology, have been supplemented by an exhaustive and critical review of the literature and translated by skillful writing into a model monograph. This is a solid reference book, and at the same time the inclusion of many tables and of an appendix on media and methods has produced a practical guide for the laboratory mycologist, physician, and pathologist.

The book is well balanced and complete. One is impressed by the attention the authors have given to clinical variations of cryptococcosis, its complex association with and relationship to lymphomas, leukemias, and sarcoidosis, the dangers of cortisone therapy, and the critical and exceptionally well-illustrated interpretations of histopathology. Discussions of the typical minimal host tissue reactions to the presence of *Cryptococcus*, the rarity and inconspicuous appearance of macrophages in some advanced lesions, the integrity of delicate cellular elements in intimate contact with *Cryptococcus*, and the eventual rupture of such structures as the walls of pulmonary alveoli under stress of pressure and sheer numbers of fungus cells of an expanding colony of the fungus, demonstrate clearly that *Cryptococcus* has no lytic activity. Many critical studies have shown the error of Stoddard and Cutler's interpretation, but the ghost of this error is still raised occasionally by uncritical observations or by failure to recognize the role of concomitant bacterial infections, as in bovine mastitis.

The authors anticipated that the recommended therapy would be soon superseded and indeed this has occurred, almost as soon as the book was published, by the availability at that time of amphotericin B. This is currently the drug of choice in treating cryptococcosis, but the need for better therapeutic agents still exists.

There are few points which require criticism. The term "botanical classification" might be replaced by "mycologic classification," although it is correct that the taxonomy of fungi follows special rules of the Botanical Rules of Nomenclature. The discussions of culture media do not present a completely fair and objective comparison.

The book contains 162 pages of text and appendix, a good combined index of subject matter and illustrations, and 504 literature references. Three pages of a table of "Contents" provide an additional useful guide to the plan of the book and the principal subjects. There are 84 figures and 4 color plates which are exceptional in excellence of photography and of reproduction. The book is of convenient size, well bound, and printed on a high quality of paper.

CHESTER W. EMMONS

COCCIDIOIDOMYCOSIS.

By Marshall J. Fiese; foreword by Charles E. Smith. Charles C Thomas, Springfield. $9.50. xiv + 253 pp.; ill. 1958.

The author states correctly that in spite of a voluminous literature on coccidioidomycosis in periodicals and textbooks, there has been no single book which includes all the information now available about this important mycosis and the fungus which causes it. The author has succeeded admirably in producing such a book. *Coccidioidomycosis*, with a foreword by Charles E. Smith, is characterized by an authoritative and inclusive presentation of the subject, clarity of writing, good illustrations, and excellent typography on good quality paper in an attractive binding.

Fiese, after discussing the currently recognized medical importance of coccidioidomycosis, records

dramatically in 10 pages the history of the mycosis, including its discovery in 1891 by Posodas, a 21-year-old medical student in Wernicke's laboratory, and the finding 50 years later of the perfectly preserved head, hand, and foot of the original patient.

Fiese also discusses adequately the mycology of *Coccidioides* both in vivo and in vitro, including its variability and biochemical characteristics. He outlines clearly the methods of isolation and study of the fungus, and while it is apparent that he believes it possible to use petri dishes safely, he warns strongly against their use. Formulas for culture media and methods of selective staining appear at appropriate places in this chapter rather than in an appendix.

Chapters on the geographic distribution of *Coccidioides* (by counties in California and by areas in other parts of the United States and the world), the epidemiology and immunology of coccidioidomycosis, and relationships of age, sex, and race to susceptibility follow. The author rejects as of no practical importance the theoretically possible role of contagion and emphasizes the importance of exposure to environmental sources of the fungus and the occasional role of fomites in exposing persons outside endemic areas. The author discusses the pathogenesis and pathology of the mycosis and, in considerable detail, the three clinical types of the disease. A short chapter is devoted to naturally acquired coccidioicomycosis in lower animals. In a final 12-page chapter, Fiese discusses supportive, drug, and surgical therapy of the mycosis, and concludes with the philosophic observation that just as in many other diseases therapy is the last frontier of coccidioidomycosis. A bibliography arranged chronologically by years from 1892 to 1957 includes 976 titles (including 8 insertions); and author and subject indices follow.

This is an excellent and dependable reference work and textbook. The principles of epidemiology, now so well established for coccidioidomycosis, are prototypes for known or suspected similar characteristics of histoplasmosis, cryptococcosis, blastomycosis, and other mycoses. Fiese has demonstrated the utility of a well-written monograph by a clinician and investigator with a first-hand knowledge of his subject.

CHESTER W. EMMONS

ÉRYTHROCYTES ET ÉRYTHROPATHIES.

By P. Cazal. Masson & Cie., Paris. 5,000 fr. ix + 627 pp. + 6 pl.; text ill. 1957.

A tremendous advance in our knowledge of erythrocytes has been made in recent years. In *Érythrocytes et Érythropathies*, the author has accumulated valuable and pertinent information about these highly complex and specialized cells in health and disease. From the title, the reader might think that the book deals solely with a study of erythrocytes (mature red cells). However, there is a good deal of information about the erythroid precursors in normal and abnormal states.

The book is divided into 14 chapters. The first 2 chapters are devoted to the study of quantitation, morphology, composition, and physico-chemical properties of erythrocytes. The third and fourth chapters deal with erythropoiesis and the factors involved. The discussion of anemia of various origins, such as specific deficiencies in important factors, hemolytic anemia, and aplastic anemia, is the subject of the following 8 chapters. Finally, Chapters 13 and 14 are devoted to the description of erythroleukemia and polycythemia.

In the analysis of the pathological states more emphasis is placed on physio-pathology; less discussion is devoted to the clinical aspect of the disease. The references, although sparse, are well selected. The bibliography, unlike most, is located in footnotes at the bottom of each page. In this fashion the reader can refer more easily to the articles in which he is interested. The book is well written, and is recommended to those with special interest in the subjects treated.

NASROLLAH T. SHAHIDI

AVIATION MEDICINE. *Selected Reviews. AGARDograph No. 25.*

Edited by Clayton S. White, W. Randolph Lovelace II, and Frederic G. Hirsch. Advisory Group for Aeronautical Research and Development North Atlantic Treaty Organization; Pergamon Press, New York, London, Paris, and Los Angeles. $12.00. viii + 305 pp.; ill. 1958.

This book is a very readable and informative series of papers on selected topics. The papers are essentially reviews of instrumentation and related methodology applicable to a wide range of medical research areas. None were meant to be exhaustive treatments of the techniques discussed, but several are outstanding scholarly papers of great value for reference on the particular subject. Some, however, are so brief as to be, for practical purposes, virtually useless. The title suggests that the reviews pertain particularly to aviation medicine. It will be found, however, that they are in no sense confined to that field and are of such general interest that the title, which implies this restricted outlook, is unfortunate.

Only one paper, that on the measurement of atmospheric ozone by Gerald Bowen, seems properly circumscribed by the title *Aviation Medicine*. Five of the twelve papers pertain to respiratory research.

The two papers presented by Clayton S. White on the analysis of respiratory gases, one of which was in collaboration with W. Randolph Lovelace on spectrometric methods, are very eloquent inquiries into the methods used to analyze respiratory gases. The authors are careful to point out the limitations of the different methods and to give adequate references for each method for further inquiry. They discuss in some detail the methods used, of which a wide variety are listed, and in particular those relating to emission analysis, with which they have had extensive experience.

The chapter by Arnold Reif, on aerosols, is also a very excellent treatment of its subject. Reif reviews most of the useful properties of aerosols along with the applicable theory and related instrumentation techniques used in quantitative measurements. Although no attempt was made to cover the chemical properties, production, or biological effects of aerosols, this chapter and the one by White, previously mentioned, are the outstanding ones in the volume.

John L. Howarth, in a less extensive but lucid article, discusses the general principles of ionizing instrument chambers and ionizing radiations. The paper by Bowen, previously mentioned, on the measurement of atmospheric ozone, describes a few typical methods for the measurement of ozone and gives appropriate references for detailed inquiry. Included also are some data on ozone determinations with varying altitude. Bowen points out that there is some question whether ozone toxicity is a real problem to aviation medicine; that work is needed to determine, in actual practice, the levels of cabin ozone occurring in high altitude aircraft, and to examine the physiological effects, if any, resulting from exposure to such concentrations as might be found in such an investigation.

There are also included papers on high speed motion picture photography; methods and apparatus for studying stress reactions; principles and practice of temperature measurements; design of electromagnetic transducers; and a very non-aviation-minded paper on technical developments applicable to problems in pathology. These papers are all fairly short, readable and informative, although somewhat restricted in usefulness.

H. G. Wagner

Air Pollution Handbook.

Edited by Paul L. Magill, Francis R. Holden, and Charles Ackley. McGraw-Hill Book Company, New York, Toronto, and London. $17.50. xii + 645 pp.; ill. 1956.

The intention of this volume is to provide a book which examines the contemporary problem of air pollution in a comprehensive manner and to bring together the several disciplines that bear on the problem. Each of the chapters was prepared by a different contributor and is meant to be complete and meaningful by itself. The contents are as follows: Air pollution sources and their control; City-planning, industrial-plant location, and air pollution; Chemistry of contaminated atmospheres; Physics of the atmosphere; Evaluation of weather effects; Visibility and air pollution; Epidemiology of air pollutants on farm animals; Effect of air pollution on plants; Sampling procedures; Analytical methods; Experimental test methods; Equipment and processes for abating air pollution; and Air pollution control by legislation.

Environmental Sanitation.

By John A. Salvato, Jr. John Wiley & Sons, New York; Chapman & Hall, London. $12.00. xiv + 660 pp. + 1 folding chart; ill. 1958.

The title of this volume, as the author promptly acknowledges in his Preface, is far more comprehensive than its content. The text deals primarily, if not exclusively, with the application of sanitary and public health engineering principles to the solution of problems of communities of 1000 to 5000 persons or less. It is directed, therefore, to the field practitioner who is often in need of technical support, easily available in a text characterized by simplicity and practicality.

In the preparation of such a text many concessions are knowingly made to oversimplification, while reasonable adherence to sound theory and practice is hoped for. The author manages to travel this middle road with some success. In doing so, however, he places a sharp limitation on his field of readers. The sophisticated professional will find it readable but less rewarding than other textbooks. The field worker of less professional background will undoubtedly find it currently useful on his bookshelf.

It is still an almost insurmountable task to cover environmental sanitation in a volume even of 650 pages. Even so, this book should help the man struggling with the sanitary problems of the small town, camp, motel, and all the other manifestations of the modern United States.

Abel Wolman

Meprobamate and Other Agents used in Mental Disturbances. *Ann. N. Y. Acad. Sci., Vol. 67, Art. 10.*

Edited by Otto v. St. Whitelock. The New York Academy of Sciences, New York. $4.00 (paper). Pp. 671–894; ill. 1957.

At present, the treatment of most mental disturbances can be handled only symptomatically, and until medical science has fathomed the depths of the central nervous system, such *relief* as can be afforded the mental patient will continue to be only symptomatic. Aldous Huxley, in reviewing the *History of Tension*, has stated: "These tranquilizing drugs are merely the latest additions to a long list of chemicals that have been used from time immemorial for changing the quality of consciousness, thus making possible some degree of self-transcendence and a temporary release from tension." This New York Academy of Sciences Conference on meprobamate and other agents now used in treating mental disturbances has attempted to evaluate the ataractic drugs as to the mechanism of their pharmacological action and their clinical efficiency. The conference is divided into 4 sections: Chemistry, Pharmacology and Mode of Action of Meprobamate; Treatment of Psychoneurotic Conditions; Treatment of Psychiatric and Other Conditions with Meprobamate; and Use of Meprobamate in Muscle Spasm. This is a comparative summary of the action of a group of drugs now employed as tranquilizing agents. The tranquilizing drugs are a mixed group which, as Berger points out, in pre-tranquilizer days have been classified quite differently, in accordance with some other pronounced pharmacological action. This point is well taken, and although the present application is primarily that of relieving the symptoms of mental disturbance, a comparative study of these mechanisms and other actions of these drugs may eventually form a more sound rationale for their use in mental disease.

THOMAS E. NELSON, JR.

INVOLUTIONAL MELANCHOLIA. *An Etiologic, Clinical and Social Study of Endogenous Depression in Later Life, with Special Reference to Genetic Factors. Acta psychiat. Kbh., Suppl. 127, Vol. 34.*

By Åke Stenstedt. Ejnar Munksgaard, Copenhaven. D. kr. 65 (paper). 71 pp. 1959.

HUMAN BIOLOGY

CONSTITUCION HUMANA Y DETERMINACION DEL SEXO. *By Milciades Martinez Gustin. Editorial Litografia, Bogotá.* $2.50. 171 pp.; ill. 1956.

One cannot fail to be impressed by the vigor with which the author of this book advances his opinion that the success of the X- or Y-bearing sperm is determined by the morphological, physiological, and psychological conditions of the parents at the time of conception. The common assumption that sex is determined by chance is stated to be incorrect and it is made to seem obvious that a comprehensive study will solve this age-old problem.

As a first attempt in this direction, Martinez has drawn a sample of families in which the children are all of the same sex. A simple index based on the relative height and weight of the parents is found to predict the sex of the offspring with infinitesimal error, although the author, modestly, claims less value for the method than is obvious from the results. One may summarize the findings as indicating that children have the sex of the parent who is relatively thinner. As if such findings needed proof, confirmation is derived from an old study by Davenport, who published data on the height and weight of parents and the sex of their children. These data of Davenport show an excess of boys where the father is thinner and an excess of girls where the mother is somewhat thinner. Where the mother is by far the thinner, there is a strong excess of boys, but this has not been noticed by Martinez. It's too bad that these data don't confirm his observations in the manner that he claims. Better luck next time.

HERMAN M. SLATIS

FROM STERILITY TO FERTILITY. *A Guide to the Causes and Cure of Childlessness.*

By Elliot E. Philipp. Philosophical Library, New York. $2.75. 120 pp.; ill. 1957.

This small volume, by a member of the Royal Council of Obstetrics and Gynecology of London, was written for the encouragement of the sterile couple desperately desirous of having offspring. If it seems a bit over-sanguine, this impression is partly due to the sympathetic tone of the author: "For those who want a family there is strong hope."

The book is understandably written for the layman and includes some details of the workup techniques used by the fertility specialist. Considerable attention is devoted to matters of common-sense psychology: "Many doctors give their patients large doses of vitamins, recommended holidays. . .," all of which "smacks a little of hocus-pocus but which sometimes succeeds remarkably well." There is a sensible chapter on adoption, recommended if treatment fails or if permanent sterility has been demonstrated at the examination. "This book," the author says, "should be able to save the doctor giving very lengthy explanations to his infertility patient as to what he is setting out to do and to serve the patient having to ask the doctor questions." The volume seems well adapted to these objectives.

CARL G. HARTMAN

THE HUMAN FIGURE.

By John H. Vanderpoel. Dover Publications, New York. $1.45 (paper). 143 pp.; ill. 1958.

A book of drawings by an artist written for other artists. The authors approach is somewhat more geometric than that of others, and leans heavily on planes and geometric forms. The text describes the various parts of the human body in terms of the artists' problems, and is aided by marginal and full-page drawings. It contains nothing of interest to the general biologist.

F. N. LOW

THE OLDER POPULATION OF THE UNITED STATES. *Census Monograph Series.*

By Henry D. Sheldon and Clark Tibbitts. Social Science Research Council in cooperation with the U. S. Dept. of Commerce, Bureau of the Census; John Wiley & Sons, New York; Chapman & Hall, London. $6.00. xiv + 223 pp.; ill. 1958.

This monograph clearly demonstrates the use of statistical data in the study of the biological phenomenon of aging. H. D. Sheldon has analyzed not only census data, but also observations from related surveys to show the influence of various factors on the age structure of the population as well as the position of older people with respect to employment, living arrangements, and income. Wherever possible, historical trends in these characteristics are recorded. Extensive appendices describe in detail the characteristics of the data used in the analysis.

The author presents a number of striking conclusions. For example, the increase in the proportion of aged (over 65) in the population of the U.S.A. between 1900 (4.1%) and 1950 (8.1%) is more a function of decreasing birth rates than of the decline in mortality. Voluntary retirement for reasons of health accounts for approximately 50% of retirements. Age-compulsory retirement systems account for only 10–15 per cent of the retirees, but other involuntary retirement, such as age-associated difficulties in finding a new job, accounts for another 35% of retirees. Actually, almost 40 per cent of males over age 65 were still working in 1950. The importance of declining health of the aged in determining employment, income, and living arrangements is stressed.

Although one may quibble with some of the conclusions drawn, Sheldon has performed a real service in assembling in one place a great deal of scattered data that are of great importance to gerontology.

N. W. SHOCK

BIOMETRY

HANDBOOK OF COMPUTATIONS FOR BIOLOGICAL STATISTICS OF FISH POPULATIONS. *Bull. No. 119.*

By W. E. Ricker. Fisheries Research Board of Canada, Ottawa. $5.00. 300 pp.; ill. 1958.

Persons studying populations will welcome this new version of Ricker's classic paper on vital statistics, originally published in 1948. The revisions and additions are sufficient to justify the new title. The first chapter begins, as it should, with a clear set of definitions that permit the reader to know exactly what is meant by each term and symbol. This section is particularly necessary in the confused area of mortality terms. The rest of the volume states principles and cites actual examples. The problems concern the estimation of mortality, the estimation of population from recaptures within a year or in different years, and a consideration of the effect of variation in survival rates on these estimates. The examples are chosen exclusively from research on fish and include, fortunately, some classics now out of print as well as some unpublished material. The number of invented examples is small. The research worker will find that he must follow the cases "with pencil and paper" to understand them, for they are not written for the novice either in mathematics or in the study of populations. Students of other populations will regret that a comparison with demographic concepts was not included. However, the book is a masterpiece of exposition and practical value.

DAVID E. DAVIS

THE MATHEMATICAL THEORY OF EPIDEMICS.

By Norman T. J. Bailey. Hafner Publishing Company, New York. $6.75. viii + 194 pp. 1957.

The bibliography included in this book contains approximately 100 titles of books and articles relating to the mathematical approach to epidemic processes. The earliest reference date is 1840, when Farr's *Progress of Epidemics* was published; and nearly half of the references are to studies published since 1950. Epidemics have always been of intense interest to man. Their study in terms of mathematical models is relatively new and is clearly of increasing interest. This is in part a reflection of the growing application of mathematics to biology and in part to the development of stochastic methods which are required for certain aspects of this problem.

Norman Bailey has been responsible for a number of the important contributions to this field. His very

welcome book, the first to be devoted solely to the mathematical theory of epidemics, brings together a historical review of the subject and various deterministic and probability models of his own and of others dealing with the single epidemic, recurrent epidemics, and endemicity.

In his introduction the author states, "Those readers who are primarily mathematicians and statisticians should find little difficulty anywhere. At the same time it is hoped that much of the book will be of value to mathematically inclined biologists, epidemiologists, and medical research workers. Since these may not all wish to study the theory in detail, an attempt has been made in each chapter both to explain the general methods of investigation adopted and to make clear the practical consequences that follow."

The presentation deals with the simplest cases first, based on homogeneous mixing of the population, immediate and continuous infectiousness after becoming infected, and equal infectiousness and susceptibility, respectively, of the infected and noninfected individuals. From this beginning a series of increasingly complicated situations is considered, such as individual variability of susceptibility, different probabilities of contact, seasonal variation in infection rate, allowance for incubation period, for geographical spread, for changes in population number, and for probability effects involving chain reactions. Most of these problems are currently insoluble except in approximate terms, but approximate solutions are given and their limitations are discussed.

The author seems over-optimistic in his assessment of the reader's ease in following the mathematics. Much of the presentation is very condensed and is accompanied by a minimum of explanation. Interpretations of the methods and results for medical research workers are not as plentiful as the introduction promises and are not entirely successful, since these paragraphs refer back to the theory in mathematical rather than medical terms and will not be understood if the mathematics has not been followed.

A final discussion of the results and their interpretation stresses the importance of observations to test the theories proceeding hand in hand with the mathematical developments. At present, detailed epidemiological data of the type required to test the theories are almost nonexistent. The type of data needed includes the number of persons according to state of susceptibility, the date of becoming infected, the time and duration of infectiousness, and the duration of immunity. Even approximations to such information are rare. One of the great merits of the attempts to set up mathematical models is that they point to gaps in observational data, which may later be filled. In the present volume this final discussion, as well as the theoretical developments which precede it, should prove stimulating to epidemiologists and laboratory scientists, as well as to applied mathematicians.

MARGARET MERRELL

DE OMNIBUS REBUS ET QUIBUSDAM ALIIS

THE WORLD OF THE ELECTRON MICROSCOPE. *Trends in Science. Volume 1.*

By Ralph W. G. Wyckoff. Yale University Press, New Haven. $5.00. xxiii + 164 pp. + 16 pl.; text ill. 1958.

The Yale University Press has just initiated a new series of books for the general reader called *Trends in Science*, the authors being drawn from authorities in various fields. The present volume is the first to be issued, and the choice is an admirable one. The author needs no introduction to the scientific world, and the contents represent an expansion of a Sigma Xi lecture given at New Haven.

The electron microscope has extended our vision into a new world of minute dimensions. Combined with the ancillary techniques of thin sectioning and shadow-graphing, it occupies a position somewhere between the conventional light microscope and the giant accelerators which probe the hearts of atomic nuclei. Its place in biology has been of inestimable value in bringing closer together the perplexing problems of structure and function, of revealing the beautiful organization of cells at submicroscopic levels, and of giving the viruses and various macromolecules a morphological identity. As the use of the electron microscope passes from description to experiment, we can expect its great potentiality to be more fully realized than it is at present.

Wyckoff describes these aspects with simple but sound exposition. If other volumes in the *Trends in Science* series are of comparable quality, the Yale University Press is off to a good start.

C. P. SWANSON

THE QUARTERLY REVIEW OF BIOLOGY publishes critical reviews of recent researches in all of the special fields of biological science. The contribution should present a synthesis or digest of the researches and a critical evaluation of them. A mere synopsis of the literature without evaluation or synthesis is not desirable.

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Material ordinarily taking the form of footnotes is set in small print and placed in the text and consequently should be written in a style so as to fit readily into the text. Acknowledgments are printed in the text in small type at the end of the article just preceding the List of Literature. Recent issues of the Quarterly should be examined for style as regards (1) section or subsection headings in the text, (2) literature citations in the text, and (3) List of Literature.

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